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THE DYNAMICAL THEORY OF RESONATORS

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ABSTRACT. It is well known that the theory of the small resonator provided with a neck communicating with the open air depends upon the assumption that the air within the neck may be treated as an incompressible fluid. In § 2 of the present paper the theory is extended to include generally necks of variable cross-section. Again, Rayleigh develops three theories depending upon the assumptions made as to the velocity-distribution of the air at the open end of a resonator. It is shown in § 3 that his theory, which assumes a variable velocity-distribution over the open end, is erroneous, and a theory which appears to be correct is put forward. The theory of the neck of variable cross-section is applied to the Helmholtz resonator in § 4. In § 5 the theory of an open end of rectangular cross-section is considered. In § 6 the general theory of resonators is dealt with. It is shown that impedance principles require modification for certain types of resonator, although they hold for most of the common types.

§ 1. INTRODUCTION

MY attention was attracted to the theory of resonators in connexion with another subject, and from the theoretical point of view it seemed to be of great interest. It soon appeared to me that the theory was imperfect and defective in several respects, and in the present paper I have endeavoured to the best of my knowledge to revise it briefly. I have not touched upon the experimental side, but I think it will be found that some of the results lend themselves readily to experimental verification. Briefly stated, the problems considered in this paper are concerned with the dependence of the characteristics of a resonator

(1) upon the distribution of normal velocity over the end of the channel which communicates with the outside air,

(2) upon the form of the channel which, in an important class of resonators, is a neck of variable cross-section, and

(3) upon the shape of the orifice, or open end, for which the impedances can be calculated in certain cases.

Rayleigh's work, fundamental though it is, requires some reconsideration. Reference may be made to the theory of the pipe of uniform circular section with an open end, which is discussed in Appendix A at the end of his *Theory of Sound*, Vol. 2. An arbitrary approximate expression for the distribution of normal velocity over the open end is there assumed; but in the present paper it is shown that that portion of the fluid, throughout which Rayleigh calculates the kinetic energy, contains a plane, coincident with the open end, over which a discontinuity of pressure must exist, which Rayleigh has not considered. A full discussion of this question will be found in § 3, where reasons are given for doubting the validity of Rayleigh's conclusions as to the normal velocity distribution over an open end. For some experimental investigations of the velocity distribution over a circular aperture in a plane wall reference may be made to a paper by E. G. Richardson*.

The nature of the velocity near the edge is remarkable, but it is important to note that the experiments determine the resultant velocity at any point and not the normal velocity, which is required by the theory. It must also be noted that the values of impedance and end correction vary little for almost any assumption which may be made respecting velocity distribution.

Accordingly, in the absence of more precise information, the discussion in § 4 of the Helmholtz resonator with respect to the effect of the form of the channel is undertaken along with Rayleigh's classical assumptions†. These are (1) that the velocity at the open end is uniform, and (2) that the pressure at the open end is uniform. They are clearly the best postulates for the theory, for they provide reasonably close limits between which the true theory lies. An extended theory of resonators is considered in § 6, and leads to a discussion of the difficulties which arise in connexion with the nature of the transition from the theory of the pipe to the theory of the Helmholtz resonator, and with the theory of impedance. For the approximate theory of sound waves in horns of variable section, which is not considered in the present paper, reference may be made to Rayleigh‡ and to Webster§.

§ 2. GENERAL THEORY

Consider a pipe (Fig. 1), which may constitute the neck of a resonator, whose longitudinal section lies in the plane of the paper, and take as axis of x a line X_1X_0 which lies in this section, X_1 being the origin of x. Consider also two planes at X_1 and X_0 respectively which are both perpendicular to the axis of x. These planes intersect the longitudinal section in the points $A_1B_1A_0B_0$; they are distant L apart; and the cross-section between them is variable.

The assumptions made are (1) that the pipe is provided with an infinite flange aA_0B_0b, the space to the right of this flange and the section A_0B_0 being occupied by the open air, and (2) that the fluid contained within the pipe between the cross-sections A_1B_1 and A_0B_0 may be treated as incompressible. The latter assumption, as Rayleigh points out, is valid if the dimensions of the neck are small compared with the wave-length of the oscillations. A velocity potential ϕ is postulated for the flow within the pipe, ϕ_1 and ϕ_0 denoting respectively the values which ϕ assumes on the planes A_1B_1 and A_0B_0.

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* "The Amplitude of Sound Waves in Resonators," *Proc. Phys. Soc.* **40**, p. 206, (1928).

† *Theory of Sound*, **2**, §§ 306, 307.

‡ *Ibid.* § 265.

§ *Proc. Nat. Acad. Sc.* **5**, 275 (1919).

Let AB be any plane cross-section parallel to the end sections, and let C be a point upon AB. Consider an elementary tube of flow C_1C_0 passing through C and let the sectional area of this tube by the plane AB be dS. If dS_n denote the normal cross-section of the tube at C, and n the normal to this section, then it follows from the requirement of continuity that

$$\frac{d\phi}{dx}.dS = \frac{d\phi}{dn}.dS_n.$$

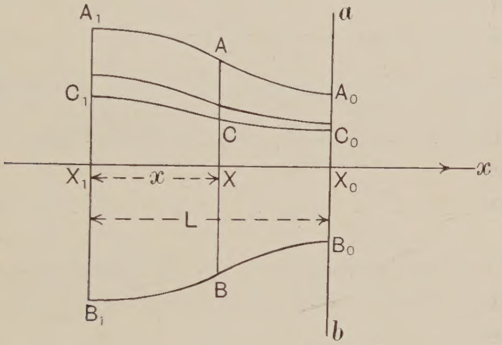

Fig. 1. Longitudinal section of neck of resonator.

Hence $d\phi/dx.dS$ is a constant for the elementary tube C_1C_0, and therefore, if ϕ_m be the mean value of ϕ over the section AB,

$$\frac{d}{dx}\left(S\frac{d\phi_m}{dx}\right) = 0 \qquad \text{......(1)},$$

where S is the area of the cross-section of the pipe by the plane AB.

From (1) it follows that

$$\phi_m = C\int_0^x \frac{1}{S}\,dx + D,$$

where C and D are constants.

Denoting by ϕ_{m1} and ϕ_{m0} the mean values of ϕ over the sections A_1B_1 and A_0B_0 respectively, we must have $D = \phi_{m1}$. Denoting also by N the integral

$$\int_0^L \frac{1}{S}\,dx,$$

we have

$$\frac{1}{N}(\phi_{m0} - \phi_{m1}) = S\frac{d\phi_m}{dx} \qquad \text{......(2)}.$$

From continuity it follows that

$$S\,(d\phi_m/dx) = S_1\,(d\phi_{m1}/dx) = S_0\,(d\phi_{m0}/dx).$$

The "neck integral," expressed by N, is the resistance of the channel.

For the kinetic energy of the fluid, of density ρ, contained between the planes A_1B_1 and A_0B_0, we have, denoting it by T,

$$\frac{T}{\rho}=\frac{1}{2}\int\frac{d\phi_0}{dx}.\phi_0\,dS_0-\frac{1}{2}\int\frac{d\phi_1}{dx}.\phi_1\,dS_1.$$

If it be assumed that ϕ_1 is uniform over A_1B_1, two cases of interest arise; (i) $d\phi_0/dx$ is assumed uniform over A_0B_0; (ii) ϕ_0 is assumed uniform over A_0B_0. In both cases

$$\frac{T}{\rho}=\tfrac{1}{2}(\phi_{m0}-\phi_{m1})\frac{d\phi_{m1}}{dx}S_1=\tfrac{1}{2}N\left(S_1\frac{d\phi_{m1}}{dx}\right)^2 \qquad \text{......(3).}$$

Again, at the open end A_0B_0, owing to the presence of the infinite flange, ϕ_0 and $d\phi_0/dx$ are connected by the important relation

$$\phi_0=-\frac{1}{2\pi}\int\frac{d\phi_0}{dx}.\frac{e^{-i\kappa r}}{r}\,dS_0.$$

Since κr is in the present paper assumed to be small, it is sufficient to write

$$\phi_0=-\frac{1}{2\pi}\int\frac{d\phi_0}{dx}.\frac{1}{r}\,dS_0+\frac{i\kappa}{2\pi}\int\frac{d\phi_0}{dx}\,dS_0,$$

and then

$$\phi_{m0}=-\frac{1}{2\pi S_0}\int dS_0\int\frac{d\phi_0}{dx}.\frac{1}{r}\,dS_0+\frac{i\kappa}{2\pi}\frac{d\phi_{m1}}{dx}S_1 \qquad \text{......(4).}$$

The resistance N. Three examples of N, the reciprocal of the conductivity, associated with common forms of the neck, are of interest. In the first case the neck is uniform so that

$$N=L/S_1.$$

For a neck of uniform circular section of radius R_1, $N=L/\pi R_1^2$, an expression obtained by Rayleigh.

In the second case

$$S=a+b\cos(\pi x/L).$$

This makes dS/dx zero when $x=0$ and $x=L$. When $a>b$ it is easy to show that

$$N=L/(S_1S_0)^{\frac{1}{2}}.$$

If the neck be of circular section, of radii at the two ends respectively R_1 and R_0,

$$N=L/\pi R_1R_0.$$

In the third case

$$S=a-bx,$$

and it follows that

$$N=L\left\{\frac{\log S_1-\log S_0}{S_1-S_0}\right\}.$$

§ 3. THE THEORY OF THE PIPE WITH AN OPEN END

In addition to the assumptions (1) and (2) of § 2 it is assumed (3) that the orifice is circular; (4) that from the section A_1B_1 (Fig. 1) to the left there is a uniform pipe, so that at A_1B_1, and to the left of A_1B_1, the waves in the pipe may be considered to be plane; and (5) that the velocity distribution over the circular end A_0B_0 is expressed by

$$d\phi_0/dx=\beta\{1+(\sigma/R_0^2)\,s^2\} \qquad \text{......(5),}$$

where β and σ are constants, R_0 is the radius of the end, and s is the distance of a representative point from the centre of the end.

It follows readily from (5), by integration over the open end, that

$$d\phi_{m0}/dx = \beta\,(1 + \sigma/2).$$

Hence
$$\beta = \frac{1}{1+\sigma/2} \cdot \frac{1}{S_0} \cdot S_1 \frac{d\phi_{m1}}{dx} \qquad \text{......(6).}$$

The method of calculating ϕ_{m0}, which is the mean value of the velocity potential induced over the surface A_0B_0 of the open end by the reaction of the external air, follows that of Rayleigh and Maxwell*.

The general result for all wave-lengths is somewhat complicated, but in the present case the problem reduces to the consideration of the first part of the right-hand side of (4). Since the calculation is an obvious extension of that of Rayleigh, it is not given here in full. The open end A_0B_0 is a circle with centre at X_0, and of radius R_0. Consider a concentric circle of radius c and let ϕ_0' be the potential at a point situated on the circumference of this circle due to that portion of the open end bounded by it.

It can be shown from (4) that

$$\phi_{m0} = \frac{4\pi}{S_0}\int_0^{R_0} \phi_0' \,.\, c\,dc + \frac{i\kappa}{2\pi}\frac{d\phi_{m1}}{dx} \,.\, S_1.$$

It can, further, be shown that

$$-\pi\phi_0' = \beta\left(2c + \frac{10}{9} \cdot \frac{\sigma}{R_0^2} c^3\right).$$

Hence it follows, from (6), that

$$\phi_{m0} = \frac{d\phi_{m1}}{dx}\left\{-\frac{8R_0}{9\pi} \cdot \frac{3+\sigma}{1+\sigma/2} \cdot \frac{S_1}{S_0} + \frac{i\kappa}{2\pi} S_1\right\} \qquad \text{......(7).}$$

But, from (2),

$$\phi_{m0} = \phi_{m1} + NS_1\,(d\phi_{m1}/dx).$$

Hence
$$\phi_{m1} = \frac{d\phi_{m1}}{dx}\left\{-\frac{8R_0}{9\pi} \cdot \frac{3+\sigma}{1+\sigma/2} \cdot \frac{S_1}{S_0} - NS_1 + \frac{i\kappa}{2\pi} S_1\right\}$$
$$= \frac{d\phi_{m1}}{dx}\{-A_0 + iB_0\}, \text{ for brevity.} \qquad \text{......(8).}$$

It is assumed for the velocity potential ψ_m within the pipe to the left of A_1B_1, the origin being at X_1 (Fig. 1), that

$$\psi_m = (A \cos \kappa x + B \sin \kappa x)\, e^{i\kappa ct},$$

corresponding to $\phi_{m0}e^{i\kappa ct}$ at the open end. It follows that

$$A = \phi_{m1} \text{ and } \kappa B = d\phi_{m1}/dx.$$

Hence
$$\psi_m = (d\phi_{m1}/dx)\,\{(-A_0 + iB_0) \cos \kappa x + \kappa^{-1} \sin kx\}\, e^{i\kappa ct},$$

the real part of which is

$$(d\phi_{m1}/dx)\,\{(-A_0 \cos \kappa x + \kappa^{-1} \sin \kappa x) \cos \kappa ct - B_0 \cos \kappa x \sin \kappa ct\}.$$

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* See also *Phil. Mag.* **46**, 434 (1923).

The equation for ψ_m is most conveniently written in the form

$$\psi_m = (d\phi_{m1}/dx)\,\{\kappa^{-1}\sec\kappa\alpha_1 \sin\kappa\,(x-\alpha_1)\cos\kappa ct - (\kappa/2\pi)\,S_1\cos\kappa x\,.\sin\kappa ct\} \quad \ldots(9),$$

where $\tan\kappa\alpha_1 = \kappa A_0$.

When κA_0 is small the end correction $(\alpha_1 - L)$ is $(A_0 - L)$, or

$$\frac{8R_0}{9\pi}\,.\,\frac{3+\sigma}{1+\sigma/2}\,.\,\frac{S_1}{S_0} + NS_1 - L.$$

If the pipe be of uniform section up to the end A_0B_0, this reduces to

$$\frac{8R_0}{9\pi}\,.\,\frac{3+\sigma}{1+\sigma/2} \quad \ldots\ldots(10).$$

The problem so far is quite definite, but it is not easy to proceed to the determination of the velocity distribution σ by theoretical considerations only. It is possible, however, to compare the result directly with that obtained by Rayleigh. Let us, therefore, consider Rayleigh's theory which is given in Appendix A at the end of his *Theory of Sound*, Vol. 2. This theory was evolved in order to determine approximately the velocity distribution over an open end. Rayleigh finds two solutions, one for the fluid inside the pipe and another for the fluid outside the pipe, the latter being fitted with an infinite plane flange; both fluids are treated as incompressible. These solutions are determined so as to give the same velocity distribution over the open end, this distribution being expressed by a function with coefficients to be evaluated. Rayleigh then proceeds to calculate the kinetic energies of each of these motions, and argues that the coefficients of the function which expresses the velocity distribution can be obtained by making the sum of the kinetic energies a minimum.

This sum represents the total kinetic energy of the motion, and it is clear that, if it contains the coefficients of the postulated velocity distribution, the pressure must be discontinuous upon crossing the plane of the open end.

In this problem it is not permissible to have recourse to diaphragms or pistons, and therefore the introduction of a pressure discontinuity at points situated in the open end A_0B_0 does not seem to be tenable.

Refer now to equation (11) on p. 490 of the *Theory of Sound*, Vol. 2. The fraction on the right of that equation becomes, on putting $\mu' = 0$,

$$\frac{0{\cdot}0666667\mu - 0{\cdot}0122728\mu^2}{(1+\mu/2)^2},$$

in which μ is the same as our σ. Rayleigh states that the value of μ required is that which makes this expression a maximum, for then the kinetic energy will be a minimum. This leads to the equation

$$\mu = 1{\cdot}152.$$

On p. 491 the corresponding value of the end correction is given as $0{\cdot}828146R$, where R is the same as our R_0. But, if this value for the end correction be equated to the expression (10) of the present paper, the corresponding value of σ turns out to be $0{\cdot}159$. This result appears to be more probable than that of Rayleigh, and it supports the criticism which was raised by Bosanquet against Rayleigh's theory.

§ 4. THE HELMHOLTZ RESONATOR

In addition to the two assumptions of § 2 it is further assumed (3) that the end A_1B_1 of the neck connects with a small sound box, throughout which the pressure rises and falls uniformly. This assumption is implied in the definition of a Helmholtz resonator. It is also assumed (4) that the whole kinetic energy of the interior fluid is confined to that contained in the neck.

Case A. This involves the further assumption (5) that the velocity distribution over the orifice is uniform. For the total pressure over the open end A_0B we have

$$S_0 p_{m0} = + S_0 \rho \, (d\phi_{m0}/dt),$$

if ρ be the density of the air. We shall write

$$\phi_{m0} = C_0 \, (d\phi_{m1}/dx) = - C_0 \, (dx_{m1}/dt),$$

in which x_{m1} is the mean fluid displacement at A_1B_1 at any instant.

For the kinetic energy we have, from (3),

$$T = \tfrac{1}{2}\rho N S_1^2 \, (dx_{m1}/dt)^2,$$

and for the potential energy of the air in the sound box

$$V = \tfrac{1}{2}\rho c^2 \, (S_1^2 x_{m1}^2/W),$$

where c is the velocity of sound, and W the volume of the small sound box. For the equation of motion it follows that

$$(d^2x_{m1}/dt^2)\,(NS_1^2 - C_0 S_0) + x_{m1} \,.\, c^2 S_1^2/W = 0 \qquad \text{......(11).}$$

Since there is dissipation, $i\kappa$ in (4) must be replaced by γ, where γ is complex. Also $x_{m1} \propto e^{\gamma c t}$.

It follows readily that

$$\gamma^3 - \gamma^2 (A + C) - B = 0, \qquad \text{......(12),}$$

where

$$A = 2\pi N S_1/S_0,$$

$$B = 2\pi S_1/W S_0,$$

and

$$C = \frac{1}{S_0^2} \int dS_0 \int \frac{1}{r} \, dS_0.$$

In all cases we may put

$$\gamma = i\kappa - \epsilon,$$

where ϵ is small, for then we obtain, approximately,

$$\kappa^2 = B/(A + C),$$

and

$$\epsilon = B/2\,(A + C)^2.$$

If n be the resonant frequency, it follows that

$$\frac{1}{\epsilon c} = \frac{4\pi}{n^4} \cdot \frac{S_1}{S_0} \cdot \frac{c^3}{W},$$

this is the time in which the amplitude is diminished in the ratio $e/1$, and, when the neck is of uniform section, it reduces to the result given by Rayleigh on p. 194 of his *Theory of Sound*, Vol. 2.

Case B. Instead of assumption (5) in case A, it is assumed (5) that the pressure over the orifice is uniform. In case A we found a cubic in γ where

$$\gamma = i\kappa - \epsilon,$$

and a solution was obtained upon the assumption that ϵ is small in comparison with κ. In the present case we commence by writing

$$\phi_{m0} = A'ce^{\gamma ct},$$

where A' is real.

It follows that

$$S_0 p_{m0} = S_0 \rho A' c^2 \gamma e^{\gamma ct}.$$

Hence, as in case A, for the equation of motion, we obtain

$$NS_1^2 (d^2x_{m1}/dt^2) + (c^2S_1^2/W)\, x_{m1} + S_0 A' c^2 \gamma e^{\gamma ct} = 0.$$

Let

$$x_{m1} = B' e^{\gamma ct};$$

then

$$(B'/A')\,(NS_1^2\gamma^2 + S_1^2/W) + S_0\gamma = 0 \qquad \text{......(13).}$$

If

$$e^{\gamma ct}(\alpha + i\beta) = d\phi_0/dx,$$

$$e^{\gamma ct}\frac{1}{S_0}\int(\alpha + i\beta)\, dS_0 = \frac{d\phi_{m0}}{dx} = -\frac{dx_{m0}}{dt} = -\frac{S_1}{S_0}\frac{dx_{m1}}{dt}$$

$$= -\frac{S_1}{S_0} B'\gamma c e^{\gamma ct}.$$

Hence

$$\int(\alpha + i\beta)\, dS_0 = -S_1 B' \gamma c \qquad \text{......(14).}$$

As before let $\gamma = i\kappa - \epsilon$; then

$$\frac{1}{2\pi}\int(\alpha + i\beta)\left(\frac{1}{r} - i\kappa + \epsilon\right) dS_0 = -A'c.$$

This requires that

$$\frac{1}{2\pi}\int\left(\frac{\alpha}{r} + \alpha\epsilon + \kappa\beta\right) dS_0 = -A'c,$$

and

$$\int\left(\frac{\beta}{r} + \beta\epsilon - \kappa\alpha\right) dS_0 = 0.$$

We assume provisionally that $\beta\epsilon$ is so small that it may be neglected.

Let

$$\int\frac{\alpha}{r}\, dS_0 = -2\pi\mu A'c.$$

Then

$$\int\alpha dS_0 = -2\pi\mu A'cE,$$

where E is the electrostatic capacity of a metal disc which has the shape of the orifice and is equal to c/π, where c is the conductivity of the orifice.

Hence

$$\int\frac{\beta}{r}\, dS_0 = -2\pi\kappa\mu A'cE,$$

so that

$$\int\beta dS_0 = -2\pi\kappa\mu A'cE^2.$$

The assumption respecting $\int\beta\epsilon dS_0$ is therefore justified. It follows at once, on neglecting κ^2E^2, that

$$\mu = 1/(1 + \epsilon E).$$

From (14) we have

$$\frac{B'}{A'} = \frac{2\pi\mu E}{S_1\gamma}(1 + i\kappa E).$$

Hence, from (13),

$$(-\kappa^2 - 2i\kappa\epsilon + \epsilon^2)(A + 1/\mu E + i\kappa EA) + (1 + i\kappa E) B = 0 \quad \text{......(15)},$$

where A and B have the same meanings as in case A.

Equation (15) leads, approximately, to the equation

$$\kappa^2 - \epsilon^2 = B/(A + E^{-1})$$

and

$$2\epsilon (A + E^{-1}) = \kappa^2 - \epsilon^2.$$

It follows that, since κ is small in comparison with $(A + E^{-1})$, ϵ must be small in comparison with κ. Hence we obtain, just as in case A,

$$\kappa^2 = B/(A + E^{-1})$$

and

$$\epsilon = B/2(A + E^{-1})^2.$$

If S_0 becomes small in comparison with S_1

$$\kappa^2 \rightarrow 1/WN,$$

which is independent of the shape of the orifice, and of the velocity distribution over the orifice. This, of course, applies also to case A.

§ 5. THE ORIFICE

The constants C and E have been left undetermined. The constant E is known for an elliptic aperture. The constant C was calculated by Rayleigh for a circular aperture. We shall complete the analysis by calculating C for a rectangular aperture.

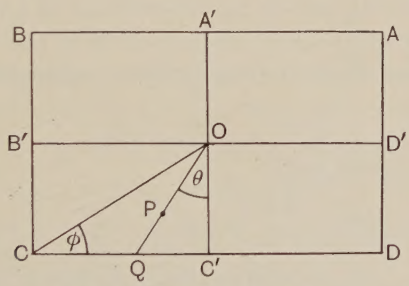

Fig. 2. Rectangular orifice.

In Fig. 2 $ABCD$ represents the rectangular open end, with sides a and b respectively. Let the angles ACD and ACB be respectively α and β. Consider a point O and through O draw the lines $A'C'$ and $B'D'$ parallel to the sides of the rectangle. From O draw a line OQ making an angle θ with OC'. Let P be a point on OQ distant r from O, and let dS_0 be an element of area at P.

Let the co-ordinates of O with respect to C be ξ, η. Let

$$I_1 = \int \frac{1}{r}\, dS_0,$$

the integration being taken over the area OCC'. It follows that

$$I_1 = \iint dr\, d\theta = \eta \int_0^{\frac{1}{2}\pi - \phi} \sec\theta\, d\theta = -\eta \log\tan(\phi/2).$$

Consider next the integral
$$H_1 = \int I_1 dS_0,$$
where dS_0 is now an element of area at O, and the integration is taken over the area ACD. Let $OC = \rho$, so that $dS_0 = \rho\, d\rho\, d\phi$.

Integrating first from O to $a \sec \phi$ with respect to ρ, we obtain
$$H_1 = \frac{a^3}{3}\int_0^\alpha \sec^3 \phi \log \tan (\phi/2)\, d(\cos \phi),$$
which, on substitution of
$$(1 - \cos \phi)/(1 + \cos \phi) \text{ for } \tan^2 (\phi/2),$$
leads to the equation
$$H_1 = \frac{a^3}{6}\left| \frac{1}{\cos \phi} + \tfrac{1}{2} \tan^2 \phi \,.\, \log \frac{1 + \cos \phi}{1 - \cos \phi} \right|_0^\alpha.$$

The corresponding integral H_2, due to a first integration over $OB'C$ and a second integration over ACD, is found likewise to be
$$H_2 = \frac{a^3}{6}\left| \tan \phi \log \frac{1 + \sin \phi}{1 - \sin \phi} - \frac{2}{\cos \phi} \right|_0^\alpha.$$

Hence, putting $H_3 = H_1 + H_2$, we have
$$H_2 = \frac{a^3}{6}\left\{\tfrac{1}{2} \tan^2 \alpha \log \frac{1 + \cos \alpha}{1 - \cos \alpha} + \tan \alpha \log \frac{1 + \sin \alpha}{1 - \sin \alpha} - \frac{1}{\cos \alpha} + 1\right\}.$$

If H_4 is due to a first integration over the whole of $OB'CC'$ and a second over ACB, H_4 is obtained from H_3 merely by substitution of b for a and β for α.

Hence $(H_3 + H_4)$ is due to a first integration over $OB'CC'$ and a second over $ABCD$. The contributions of the remaining three rectangles must all be the same. Hence
$$S_0{}^2 C = 4(H_3 + H_4).$$

If $\alpha = 45^\circ$, i.e. for a square end,
$$S_0{}^2 C = 8H_3 = \frac{8a^3}{6}\left\{\tfrac{3}{2} \log \frac{\sqrt{2} + 1}{\sqrt{2} - 1} - \sqrt{2} + 1\right\}$$
$$= (a^3/3) \times 8{\cdot}934, = S_0 a\, 8.934/3.$$

For a circular aperture
$$S_0{}^2 C = S_0 (8/3)\, 2R_0.$$

Hence the ratio of C for a square aperture to its value for a circular aperture of equal area is
$$8{\cdot}934 \sqrt{\pi}/16 = 0{\cdot}981.$$

§ 6. THE EXTENDED THEORY OF RESONATORS

A uniform pipe $ABCD$ of length L_n and cross-sectional area S_n is joined at the end CD to another uniform pipe $C'D'EF$ of length M_n and cross-sectional area T_n, Fig. 3. The end AB has a neck ABA_0B_0 of resistance N_n, the end correction measured from AB being α_n. The end EF communicates with a small junction box H. The whole resonator consists of any number of systems similar to this one, all communicating with the same junction box H.

This box being small, it is assumed that the air inside it moves as an incompressible fluid would. It is further assumed that the variation of the velocity potential within the box may be neglected: this is equivalent to neglecting the kinetic energy. The validity of this assumption is sufficiently obvious in all practical cases, and the whole approximate theory of resonators must be based upon it.

As we are considering resonant frequencies, no account will be taken of dissipation. Let x_n be measured from O (Fig. 3) to the right and y_n from Q to the left.

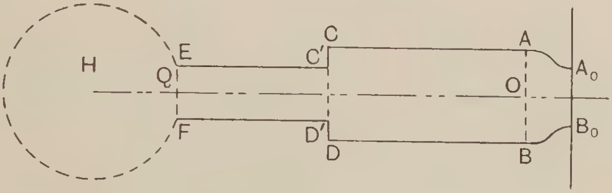

Fig. 3. Longitudinal section of element of resonator system.

Let the velocity potential for $ABCD$ be

$$\phi_n = \beta_n \sin \kappa (x_n - \alpha_n),$$

and for $C'D'EF$

$$\psi_n = \gamma_n \sin \kappa (y_n - \delta_n).$$

It follows at once that

$$\beta_n \sin \kappa (L_n + \alpha_n) = \gamma_n \sin \kappa (M_n + \delta_n),$$

and
$$S_n \beta_n \cos \kappa (L_n + \alpha_n) + T_n \gamma_n \cos \kappa (M_n + \delta_n) = 0.$$

Hence
$$S_n \cot \kappa (L_n + \alpha_n) + T_n \cot \kappa (M_n + \delta_n) = 0 \qquad \text{......(16).}$$

At the junction box we have, on account of continuity and the constancy of ψ,

$$\Sigma T_n \cot \kappa \delta_n = 0.$$

Hence, from (16),

$$\Sigma \frac{T_n^2 - S_n T_n \cot \kappa (L_n + \alpha_n) \cot \kappa M_n}{S_n \cot \kappa (L_n + \alpha_n) + T_n \cot \kappa M_n} = 0. \qquad \text{......(17).}$$

If α_{n0} be the end correction which would correspond to a uniform pipe of sectional area S_{n0}, which is the area of the open end $A_0 B_0$, we have seen that

$$\tan \kappa \alpha_n = \kappa (\alpha_{n0} + N_n S_{n0}) S_n / S_{n0} \qquad \text{......(18).}$$
$$= \kappa \epsilon_n, \text{ say.}$$

The formula (17) includes all resonators that are commonly met with.

The type $n = 1$.

From (17) we have at once

$$\tan \kappa M_1 \tan \kappa (L_1 + \alpha_1) = S_1 / T_1 \qquad \text{......(19).}$$

This is a slightly more general form of an equation given by J. A. Aldis* and discussed by E. T. Paris†.

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* *Nature*, **114**, 309 (1924).

† *Nature*, **114**, 465 (1924).

Putting $L_1 = 0$ and making κM_1 small, we get the equation for the resonant frequency of a Helmholtz resonator in the form

$$\kappa^2 = S_1/M_1 T_1 \epsilon_1.$$

The formula for the Helmholtz resonator can also be obtained by the equating of T_1 to zero in (19). In this case

$$\tan \kappa L_1 \tan \kappa \alpha_1 = 1,$$

which is the required result when κL_1 is small.

Dr Richardson, in his paper quoted in the introduction, assumes a cylindrical cavity of volume SL, S being the cross-sectional area of the cylinder and L its length. He then assumes that with the value of L for a pipe is incorporated an end correction. He states finally that, for an ideal pipe, the conductivity of the orifice is infinite. There seems to be confusion as to what the volume of the cavity is, and the meaning to be attached in his formula to infinite conductivity appears to be vague, unless the author has misunderstood him.

For a pipe we may put $S_1 = T_1$ and $L_1 = 0$ in (19) and we obtain

$$\tan \kappa M_1 = 1/\kappa \epsilon_1.$$

The type $n = 2$.

This type includes, among others, Rayleigh's double resonator, the Helmholtz double resonator, Boys' resonator, the dumb-bell resonator, the Kundt's tube. In formula (17) we make M_2 zero, so that we have three pipes. The formula then reduces to

$$S_1^{-1} \tan \kappa (L_1 + \alpha_1) + T_1^{-1} \tan \kappa M_1 + S_2^{-1} \tan \kappa (L_2 + \alpha_2)$$
$$= (T_1/S_1 S_2) \tan \kappa (L_1 + \alpha_1) . \tan \kappa M_1 . \tan \kappa (L_2 + \alpha_2) \text{ ......(20).}$$

In those cases listed above the right-hand side of (20) is small and may be neglected. For example, the formula for the Rayleigh double resonator becomes

$$\frac{1}{S_1} \cdot \frac{L_1 + \epsilon_1}{1 - \kappa^2 L_1 \epsilon_1} + \frac{M_1}{T_1} + \frac{1}{S_2} \cdot \frac{L_2 + \epsilon_2}{1 - \kappa^2 L_2 \epsilon_2} = 0,$$

which is a quadratic in κ^2.

By making ϵ_2 infinite we obtain the Helmholtz double resonator; and by making both ϵ_1 and ϵ_2 infinite we get the dumb-bell resonator.

In all cases there appears to be no dubiety as to the nature of the transition from "pipe" theory to "small resonator" theory.

The term impedance, undoubtedly of great use in many branches of acoustics, must be used with caution. Each term on the left-hand side of (20) is, with the omission of a constant factor, the impedance of the pipe to which it relates. In the examples given it is permissible to add the impedances in series and equate to zero for resonance. In the case of two pipes, as we have also seen, it is likewise permissible to do this. But, generally, in the case of three pipes the theory of impedance breaks down. The omission of the right-hand side of (20) in some cases leads to results that are entirely wrong.

§ 7. ACKNOWLEDGMENT

In conclusion I have to acknowledge my indebtedness to the Admiralty for permission to publish this paper.

DISCUSSION

Dr E. T. Paris. I have listened to Mr Hanson's paper with great interest, and think that the formula which he has given for calculating the conductance of an orifice in which the cross-sectional area varies along the length is of great practical value.

In the application of Rayleigh's frequency formula to the design of a Helmholtz resonator difficulty often arises from the indefiniteness of the end-correction which must be added to the length of the orifice. In the case of wide orifices the end-correction plays a dominant part in determining the conductance, and however accurately one can calculate this correction when there is a hypothetical infinite flange, this does not assist in overcoming the practical difficulty of knowing how much to allow for end-correction when dealing with actual resonators.

In nearly all theories of resonance the only allowance made for damping is on account of the radiation of sound-waves from the orifice; but if the latter is small by far the greater part of the damping is due to viscosity. There seems to be some inconsistency in allowing the viscous forces in the orifice to enter into the exact theory of a resonator, by determining the distribution of velocity in the orifice, and at the same time ignoring altogether their dissipation effect. A complete theory of the Helmholtz resonator in which viscosity was fully allowed for would be very valuable in the practical design of resonators.

The President asked whether Dr Paris had measured experimentally the damping due to viscosity.

Dr Paris said that in a cylindrical neck 1 cm. long and 0·5 cm. in diameter the total damping factor was found to be about 80 sec.$^{-1}$ whereas the calculated radiation damping was only about 2 sec.$^{-1}$

Dr E. J. Irons. Equation (20) of the paper is readily obtained by repeated application of equation (28) of Webster's paper to which reference is made at the end of § 1: the grounds upon which Mr Hanson criticises the theory of impedance at the end of § 6 would therefore appear to be non-existent. In the *Philosophical Magazine* for May of this year I derived on impedance principles formulae for the resonators mentioned in § 6. In a paper which I have recently submitted for publication in the same journal I have extended the argument to include compound resonators having a conical horn as a component part and I have shown how "parallel" systems such as a closed pipe with two Helmholtz resonators on the side (considered by Dr Paris in 1924) may be treated. I have also considered a general "series" system which, in a manner somewhat different from Mr Hanson's equation (20), shows under what conditions Rayleigh's formula for his double resonator may be applied.

Sir Richard Paget. In practice it appears that the pitch of a resonator with a tubular neck is not affected by the addition of a flange of flaring bell form; in other words, the "infinite flange" is not a right-angled extension of the neck but a curved surface which merges into a surface at right angles to the axis of the neck. It also appears that in the design of a resonator for a given pitch the nature of the resonator wall is very important owing to variations in transparency to sound of different materials. Sound-transparency operates to raise the resonant pitch in much the same way as if the resonator walls were perforated with small holes.

Dr E. G. Richardson (communicated). I have not had time to follow Mr Hanson's interesting paper throughout, but I note that in equation (5), for the distribution of velocity across the mouth of the resonator, his theory makes $\sigma = 0{\cdot}159$ as against Rayleigh's $1{\cdot}152$. Rayleigh's value would give peaks at the edge, viscosity being neglected, of about double the central velocity; whereas on Mr Hanson's theory the peaks would scarcely be noticeable. I should like to say that the results with the hot wire showed peaks much more pronounced than those which a coefficient of $0{\cdot}159$ would indicate. With an unflanged tube the peak velocity was about double the central velocity, so that with an "infinite flange" (to which these theories apply) the ratio of the peak to the central velocity was about 3 : 2. It is true that the hot wire measures the total velocity, but the measurements to which I refer were made just inside the pipe, where the flow is probably axial.

With regard to equation (6) of my paper, for the frequency of the pipe-resonator, Mr Hanson has given in his equation (19) a more general formula for this type. I admit that the idea of infinite conductivity is *physically* vague, but I suggest that it is a postulate necessary to an ideal pipe, i.e. one having no end-correction.

I am interested in the last paragraph of the paper, as I have been working out the theory of fingering on wood-wind instruments. Suppose you have a number of side-holes uncovered on the flute, and wish to work out the resonant frequency: does the author think that the impedance principle would break down in this case, or does he limit his stricture to the case of three or more pipes in series?

Author's reply. Dr Paris has emphasised very clearly the importance of allowing for viscosity in the complete theory of a resonator, and the figures which he gives, in his reply to the President, as to the damping which must be attributed to viscosity are very significant. An exact resonator theory is by no means easy, and in fact, in the present state of our knowledge, little more is to be gained than has already been done by Rayleigh, unless, by direct experimental investigation as to the law of resistance due to viscosity, the form and dimensions of the neck being taken into account. In the case of a Helmholtz resonator with a circular orifice of radius R, if it be assumed that there is a viscous drag proportional to $2\pi R$ and also to the mean velocity over the orifice, the theory as given in my paper or otherwise, with an obvious extension to include the drag, will readily indicate that the damping effect due to this resistance is inversely proportional to R, so that, as Dr Paris has found experimentally, for small orifices the damping effect due to viscosity is very great. It must be remembered, however, that it is easy to make assumptions upon which to build a mathematical theory and, although it seems to me that there is some evidence that the seat of the effective viscous forces is round the sides of the orifice, the assumption is too empirical to be of very great value.

In reply to Dr Irons: I agree that Webster's formula can be used as a basis from which to derive my results, but, so far as I know, Webster did not develop his theory in connexion with systems of resonators. In my study of the subject I gathered the impression that, for a system of resonators in series, the impedances of each component resonator could be added in series in order to obtain the resonant frequency, and I have been unable to find anywhere what I considered to be an adequate treatment of the subject.

My object, in § 6 of my paper, was to make clear the limits to this procedure, and I had intended to correct the impression I have given in my paper that the theory of impedance, when properly applied, breaks down. I may say that I read Dr Irons' paper in the *Philosophical Magazine* with interest, and I shall look forward to the publication of the next paper to which he refers.

Sir Richard Paget's remarks as to the pitch of a resonator with a tubular neck are very interesting, and it appears to me that they also tend to confirm that a jet motion takes place at the orifice rather than a motion, which Rayleigh favoured, resembling that of a hydrodynamically perfect fluid. For in the case of jet motion the reaction of the external air is probably little affected by the addition of a flange of flaring bell form at the end of the tube in place of an infinite plane flange.

I was particularly interested in Dr Richardson's paper, to which I have referred. The forms of the peaks which he reproduces there are noteworthy on account of their variation for frequencies varying from very slow to acoustic rapidity. These refer, I understand, to a hole in a plate. For a tube the results will be rather different, and in his reply I take it that Dr Richardson refers to a tube.

With regard to my figure of 0·159 for σ it must be remembered that this was obtained from a critical comparison with Rayleigh's theory, depending upon a precise value for the end-correction. Now, as Dr Paris has pointed out, there is considerable indefiniteness as to the end-correction which must be added to the length of a pipe. If only a slightly different value were taken for the end-correction, my value of σ would be materially altered. In fact the present state of the theory is incapable of determining the velocity distribution with a reasonable approach to accuracy. The case which Dr Richardson takes at the end of his reply in relation to fingering on wood-wind instruments is of a somewhat more general type than can be dealt with at once by my formula, but it can be treated by a method similar to mine or by an application of impedance theory. Dr Richardson admits that the idea of infinite conductivity is physically vague, and this is one of my reasons for objecting to the introduction of electrical analogies into the theory under discussion.

ESCAPEMENT ERRORS OF PENDULUM CLOCKS

By E. C. ATKINSON, M.A.

Communicated by Prof. C. V. Boys, September 16, 1929.
Read and discussed November 22, 1929.

ABSTRACT. Prof. R. A. Sampson's theory of maintenance is applied to find equations for the errors of rate caused by changes in intensity and limits of the impulse and in friction of moving parts of the impulse mechanism. As an example, the equations are worked out for the Cottingham clock and applied to find best working conditions. Rigidity of the stops limiting the impulse is found to be the most important point in designing this type. The equations are also given for the dead beat escapement and show that working conditions which are good so far as friction is concerned are bad for barometric changes. The method used for computing "remote" error in these cases must be modified when the inertia of the impulse mechanism is appreciable as in the Shortt clock. The further remote escapement errors due to changes in amplitude introduced by variation of temperature and density of the air are the same as those produced by changes in escapement but are associated with different direct errors which are excluded from this paper.

The following table of symbols will be found convenient:

πa, πb, πc	accelerations due to escapement friction.
A, B	components of maintenance vector.
$D.d\alpha/\alpha$	remote error.
E	supply of energy to pendulum per cycle.
f	oscillation frequency.
g	acceleration due to gravity.
h	coefficient of dt in maintenance integral B (§ 9).
l	distance between centre of mass and point of support of pendulum.
M	mass of pendulum.
Mk^2	moment of inertia of pendulum about its point of support.
ω	$2\pi f$ for free pendulum.
n	$2\pi f$ for maintained pendulum.
t	time measured in seconds, generally from zero when $\theta = 0$.
x	defect from final amplitude (§ 5).
X, Y	abbreviations for τ functions (§ 8).
α	amplitude.
β	small function of τ (§ 4).
δ	decrement factor, $-2\delta\dot{\theta}$ = resistance acceleration.
η	$\frac{dE}{E} \Big/ \frac{d\alpha}{\alpha}$ (§§ 3, 4, etc.).
θ	angle between pendulum and vertical.
$\lambda.d\alpha/\alpha$	amplitude error, of which circular error is a component.
$\pi\sigma$	maintenance acceleration.
τ	phase in radians ($= nt$).

The proportional change of rate is dn/n or 86400 (dn/n) (s/d).

Unless otherwise stated, angles are measured in radians, while velocities and accelerations are all angular.

§ 1. INTRODUCTION

Eleven years ago Prof. R. A. Sampson* demonstrated a general method for calculating deviations in rate produced by escapements. I wish to show in this paper how this method may be applied to finding errors in rate resulting from changes in the working conditions. In order to be brief I have excluded variations of temperature and air density although errors due to these causes also include escapement terms. For a similar reason I have concentrated on the method and refer only briefly to the use which can be made of the results.

The equations of error exhibit the merits and defects of an escapement, they point out the essential features in design, and from them the most favourable working conditions can be calculated. All recent improvements in timekeeping have been made by securing comparative uniformity in working conditions. Little attention has been directed to the problem of minimising the effect of variations which still take place.

§ 2. VECTORS

The following sketch of well-known facts will serve to introduce the symbols to be employed. In the absence of resistance and circular equation, the motion of a pendulum would be harmonic, so that the displacement θ of the rod from the vertical is represented by the projection on OX of a *displacement vector* OD of length representing the amplitude α, which rotates with angular velocity ω. $\theta = \alpha \sin \tau$ where $YOD = \tau = \omega t$. The angular velocity is similarly derived from a *velocity vector* OV of length $\omega\alpha$ rotating 90° ahead of OD. The *acceleration vector* $\omega^2\alpha$ is 180° ahead of OD. Thus $Mk^2\omega^2 = Mgl$, Mk^2, g and l having their usual meanings.

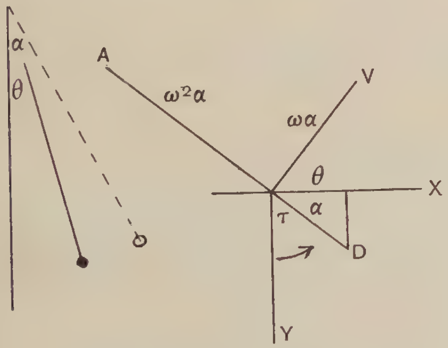

Fig. 1. Displacement, velocity and acceleration vectors.

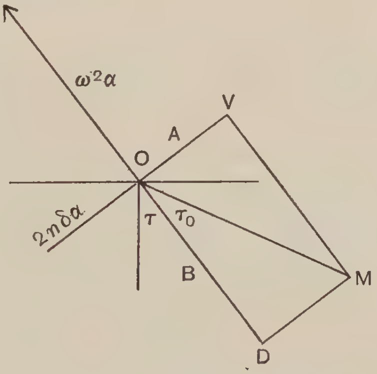

Fig. 2. Resistance and maintenance vectors.

In order that a constant amplitude may be maintained when the pendulum encounters friction, a maintaining force must be applied. Provided the acceleration due to friction were $-2\delta\dot{\theta}$† (i.e. always proportional to the angular velocity), motion would still be harmonic if the maintaining force were harmonic, having a period equal to that of the pendulum, which will now be $2\pi/n$ instead of $2\pi/\omega$. Fig. 2 shows OM, the maintenance-acceleration vector with components A, B along OV, OD respectively, and the resistance-acceleration vector $2n\delta\alpha$.

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* *Proc. R.S. E.* **38**, p. 75 (1918).

† 2δ is used for κ in order to avoid fractions.

It follows that central acceleration

$$-n^2\dot{\alpha} = -\omega^2\alpha + B.$$

Δn, or $(n - \omega)$, is the *deviation* from natural frequency and is of the order $\omega \cdot 10^{-4}$. Thus the equation $-\Delta n\,(\omega + n)\,\alpha = B$ may without appreciable error be written

$$-\Delta n/n = B/2n^2\alpha \qquad \text{......(1).}$$

It also follows from Fig. 2 that, for steady motion,

$$2n\delta\alpha = A \qquad \text{......(2).}$$

(1) and (2) are the fundamental equations of maintenance.

If the motion be steady (but *not otherwise*) $-\Delta n/n = (\delta/n)\,(B/A)$.

In calculating errors, this form of the equation is best avoided, but results are often translated into a form involving δ/n by a final multiplication by

$$2n\delta\alpha/A\ (=1).$$

If E be the supply of energy during each cycle,

$$E = \pi\,.\,Mk^2\,.\,A\alpha \qquad \text{......(3).}$$

§ 3. DECREMENT

If the resistance law assumed above were true, amplitude of the pendulum when maintenance is removed would diminish exponentially so that

$$-\frac{1}{\alpha}\frac{\Delta\alpha}{\Delta t} = \delta = \text{constant.}$$

Experiment shows that the left-hand side of this equation increases with α, and δ must be regarded, not as a constant, but as a function of α.

Differentiating (3) logarithmically after substituting $2n\delta\alpha$ for A, we find that

$$\frac{dE}{E} = \frac{2d\alpha}{\alpha} + \frac{d\delta}{\delta} = \eta_1\frac{d\alpha}{\alpha} \text{ where } \eta_1 - 2 = \frac{d\delta}{\delta}\Big/\frac{d\alpha}{\alpha} \qquad \text{......(4, 5).}$$

η_1 is never less than 2 and rarely, if ever, exceeds 3.

Decrement experiments furnish no information as to the resistance at each moment, and very little is known about this. Since the pendulum is moving in air which it has already disturbed, the relative velocity cannot in general be the same as that of the pendulum. Resistance as indicated in Fig. 2 is in "A" direction and has consequently no effect on the rate*, but it is clear that there is a "B" component as well, which (although probably smaller than $Mk^2 . 2n\delta\alpha$) affects frequency perhaps 1000 times as much: this component is not negligible†, and changes with amplitude.

Amplitude equation. The frequency of a pendulum decreases as amplitude increases since the centre of gravity moves in a circle. The deviation from the rate which would obtain with infinitesimal amplitude is called the circular equation. We have just seen that there is another deviation to which must be added change in the inertia effect of the air. Moreover spring suspension tends to reduce the circular equation, and finally lack of rigidity (or rather of perfect elasticity) of the pendulum bracket will also cause change of frequency with amplitude. It is logical to speak of the amplitude equation, leading to *amplitude error*[*], $\lambda . d\alpha/\alpha$ and to confess that, in using $(\alpha^2/8)(d\alpha/\alpha)$ in place of this, we are ignorant of the real value of amplitude error.

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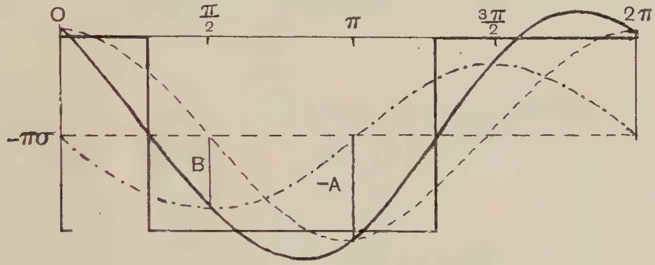
* i.e. when amplitude is maintained constant.

† Pointed out by Stokes, *Camb. Phil. Soc. Trans.* **9** (1850).

§ 4. ESCAPEMENTS. *A* AND *B* AS DEFINITE INTEGRALS

Harmonic maintenance as assumed in § 2 can be secured in electrical systems, but no escapement has yet been devised to do the same thing for the pendulum. The impulse however is always periodic, the period being $2\pi/n$. Consequently the acceleration can appropriately be expressed as a Fourier's series

$$\pi\sigma^{\dagger} = A_0 + A\cos\tau + B\sin\tau + A_2\cos 2\tau + B_2\sin 2\tau + \text{etc.}$$



$$A = \int_0^{2\pi} \sigma\cos\tau\,d\tau \qquad B = \int_0^{2\pi} \sigma\sin\tau\,d\tau$$

Fig. 3. Cottingham impulse and equivalent sinusoidal impulse with its components.

Prof. Sampson in his paper shows that, whilst A_0, A_2, B_2, etc. affect the position of the pendulum at any moment, their effect on rate and amplitude is negligible and that, in the determination of these two quantities, *A* and *B* alone need be considered. Thus equations (1) to (5) are true for escapements when

$$A = \frac{1}{\pi}\int_0^{2\pi} \pi\sigma\cos\tau\,d\tau = \int_0^{2\pi} \sigma\cos\tau\,d\tau \qquad \text{......(6)},$$

$$B = \int_0^{2\pi} \sigma\sin\tau\,d\tau \qquad \text{......(7)}.$$

In order that *A* and *B* may be computed, σ must be expressed as a function of τ. As a rule σ is (primarily at least) a function of θ and, owing to the escapement, it is no longer true that $\theta = \alpha\sin\tau$. σ should be expressed as another series and the coefficients of neither series can be computed without a knowledge of those of the other, except by successive approximation. Is the first approximation obtained by using $\alpha\sin\tau$ for θ good enough for *A* and *B*? On the assumption that

$$\theta = a + b\sin(\tau + \beta)$$

for a second approximation (β being a small function of τ) the first approximation can be checked. I find that it suffices for any reasonable value of δ provided σ does not change rapidly near the extremities of the swing. It will be noticed that in this exceptional case a small change in $\sin\tau$ (i.e. θ/α) may be accompanied by a large change in $\cos\tau$ and would appreciably affect $\int\sigma d(\cos\tau)$ if σ is changing rapidly at the same time.

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[*] Coined by J. M. Bloxham, who states that it is much smaller than the circular error. (*R.A.S. Memoirs*, **22**, 1853).

[†] $\pi\sigma$ is used to avoid the fraction $\frac{1}{\pi}$ in integrals.

This exceptional case does not occur in practice (except when the clock is on the point of stopping!).

§ 5. ERRORS; DIRECT AND REMOTE

(*a*) An error or change of deviation $-dn/n$ results from a change dB in B. From equation (1)

$$-d_d n/n = dB/2n^2\alpha \qquad \text{......(8).}$$

This is called the *direct error* because it follows immediately on the change dB. Equation (8) means that the clock is losing 86400 ($dB/2n^2\alpha$) (s/d) on its previous rate.

(*b*) When A increases by dA at the same time, energy supplied to the pendulum increases by dE_1 where $dE_1/E = dA/A$, α will commence to grow and will eventually reach $\alpha + d\alpha$. This will produce a further change in rate which I call *remote error*. E is the nett energy reaching the pendulum after the energy released by the escapement has been taxed at source by escapement friction. The tax usually increases with α, giving a remote (usually negative) change dE_2

where
$$\frac{dE_2}{E} = \frac{d(A\alpha)}{d\alpha} \cdot \frac{d\alpha}{A\alpha}.$$

Equations (3) to (5) show that

$$\eta_1 \frac{d\alpha}{\alpha} = \frac{dA}{A} + \frac{d(A\alpha)}{A d\alpha} \cdot \frac{d\alpha}{\alpha} = \frac{dA}{A} - \eta_2 \frac{d\alpha}{\alpha}$$

or

$$\eta \frac{d\alpha}{\alpha} = \frac{dA}{A}, \text{ where } \eta = \eta_1 + \eta_2 \qquad \text{......(9).}$$

After the initial change dA the energy supplied by the escapement remains unchanged except for increasing frictional losses, so that η_2 represents the ratio of dE/E to $d\alpha/\alpha$ for escapement friction, just as η_1 represents this ratio for pendulum friction. Whilst η_1 ranges from 2 to 3, η_2 should be a small fraction.

The remote error consists of amplitude error (§ 3) and a further escapement error

$$-\frac{d_r n}{n} = \left[\frac{1}{2n^2}\frac{d}{d\alpha}\left(\frac{B}{\alpha}\right) + \frac{\lambda}{\alpha}\right] d\alpha = \left[\frac{1}{2n^2}\left(\frac{dB}{d\alpha} - \frac{B}{\alpha}\right) + \lambda\right]\frac{d\alpha}{\alpha} = D\frac{d\alpha}{\alpha} \quad \text{...(10).}$$

By (9) and (2) the total error $-d_d n/n - d_r n/n$ may be written

$$-\frac{dn}{n} = \frac{\delta}{n}\frac{dB}{A} + \frac{1}{\eta}\left[\frac{\delta}{n}\left(\frac{dB}{d\alpha} - \frac{B}{\alpha}\right) + \lambda\right]\frac{dA}{A} \qquad \text{......(11).}$$

This equation is the general equation of error. A and B depend on intensity of impulse and its range and on friction in various parts of the escapement. To change in any one of these quantities, there is a corresponding error.

Transitional state. Having made some change in working conditions, we should reckon time by making allowance for the new error. The remote part of this error, however, is not present at first: when the amplitude is short by x of its final value $\alpha + d\alpha$ the remote error is only $D\,(d\alpha - x)/\alpha$, so that the clock is gaining $D\,(x/\alpha)$ seconds per second on its final rate. At this time the escapement supplies energy $\pi Mk^2\,\{(A + dA)\,\alpha + (d\alpha - x)\,.\,d\,(A\alpha)/d\alpha\}$ per cycle whilst the pendulum loses $\pi Mk^2\,.\,2n\delta\alpha^2\,\{1 + \eta_1\,(d\alpha - x)/\alpha\}$. The nett gain of energy is

$$\pi Mk^2\,.\,2n\delta\alpha^2\,\{\eta_1\,(x/\alpha) - (x/\alpha)\,d\,(A\alpha)/A\,d\alpha\}$$

per cycle or $\frac{1}{2}n\,Mk^2\,.\,2n\delta\alpha\,.\,\eta x$ per second, which increases the amplitude by $-\,dx/dt$ and the pendulum energy by $-\,Mgl\alpha\,(dx/dt)$ or $-\,n^2Mk^2\alpha\,(dx/dt)$.

Hence $$\eta\delta x = -\,dx/dt \text{ and } x = d\alpha e^{-\eta\delta t} \qquad \text{......(12).}$$

The gain in time during the transition as reckoned above

$$= \frac{D}{\alpha}\,d\alpha \int_0^\infty e^{-\eta\delta t}\,dt = \frac{D}{\eta\delta}\,.\,\frac{d\alpha}{\alpha} \text{ seconds} \qquad \text{......(13).}$$

§ 6. COTTINGHAM CLOCK

It will be seen that the following routine should be followed for calculating an error:

(1) B and $A\alpha$ are found from impulse and frictions by integration (6), (7).

(2) D is calculated, the principal terms of B only being retained (10).

(3) $\eta\,(d\alpha/\alpha)$ is computed from $A\alpha$ variations in all elements being assumed (9). For variation of each element the corresponding error equation can then be written down.

As an example, I select the Cottingham escapement in which a gravity lever-rests against a stop S connected to the armature of an electromagnet. Fig. 4 represents the state of affairs when the pendulum is about to pick up the lever. The pick up completes a circuit and S is withdrawn to S' to be returned again to S when the circuit is broken during the return swing of the pendulum.

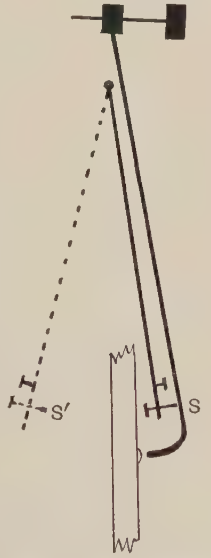

Fig. 4. The Cottingham clock.

Pivot friction increases the uniform escapement acceleration $-\pi\sigma$ to $-\pi\,(\sigma + a)$ in the range θ_1 to α. Similarly the acceleration is $-\pi\,(\sigma - \alpha)$ in the range α to θ_2. Impulse acceleration may be briefly expressed as

$$[-\pi\,(\sigma + a)]_{\tau_1}^{\pi/2} + [-\pi\,(\sigma - a)]_{\pi/2}^{\tau_2}.$$

$$\text{(1)}\quad B = -\,(\sigma + a)\int_{\tau_1}^{\pi/2} \sin\tau\,d\tau - (\sigma - a)\int_{\pi/2}^{\tau_2} \sin\tau\,d\tau$$
$$= -\,\sigma\,(\cos\tau_1 + \cos\tau_2) - a\,(\cos\tau_1 - \cos\tau_2) \qquad \text{......(14).}$$

Similarly $$A = \sigma\,(\sin\tau_1 - \sin\tau_2) - a\,(2 - \sin\tau_1 - \sin\tau_2)$$

or $$2n\delta\alpha^2 = A\alpha = \sigma\,(\theta_1 - \theta_2) - a\,(2\alpha - \theta_1 - \theta_2) \qquad \text{......(15).}$$

(2) Since

$$\alpha \sin \tau_1 = \theta_1 = \text{const.}$$

$$d\tau_1 = -\tan \tau_1 \frac{d\alpha}{\alpha}$$

$$dB/d\alpha = -(\sigma/\alpha)(\sin \tau_1 \tan \tau_1 + \sin \tau_2 \tan \tau_2)$$

$$\frac{1}{2n^2}\left(\frac{dB}{d\alpha} - \frac{B}{\alpha}\right) = -\frac{2n\delta\alpha}{2n^2\sigma(\sin\tau_1 - \sin\tau_2)} \cdot \frac{\sigma}{\alpha}\left(\frac{\sin^2\tau_1}{\cos\tau_1} - \cos\tau_1 + \frac{\sin^2\tau_2}{\cos\tau_2} - \cos\tau_2\right)$$

$$= (\delta/n)(2 - \sec\tau_1 \sec\tau_2)\cot(\tau_1 - \tau_2)/2 \qquad \ldots\ldots(16).$$

(3) $$\eta_1 \frac{d\alpha}{\alpha} = \frac{dA}{A} + \frac{d}{Ad\alpha}(A\alpha) = \frac{d\sigma}{\sigma} - \frac{da}{\sigma}\frac{2\alpha - \theta_1 - \theta_2}{\theta_1 - \theta_2} - \frac{d\theta_1 - d\theta_2}{\theta_1 - \theta_2} - \frac{2a}{\sigma(\theta_1 - \theta_2)}\frac{d\alpha}{\alpha}$$

or

$$\eta \frac{d\alpha}{\alpha} = \frac{d\sigma}{\sigma} - \frac{da}{\sigma} \cdot \frac{2\alpha - \theta_1 - \theta_2}{\theta_1 - \theta_2} + \frac{d\theta_1 - d\theta_2}{\theta_1 - \theta_2} \text{ where } \eta = \eta_1 + \frac{2a}{\sigma(\theta_1 - \theta_2)} \ldots(17).$$

(a) *Limits errors.* If θ_1 increases by $d\theta_1$, $d\tau_1 = d\theta_1/\alpha \cos \tau_1$

$$-\frac{d_a n}{n} = \frac{\sigma \sin\tau_1}{2n^2\alpha} \cdot \frac{d\theta_1}{\alpha\cos\tau} = \frac{2n\delta\alpha^2 d\theta_1}{\sigma(\theta_1 - \theta_2)} \cdot \frac{\sigma}{2n^2\alpha^2}\tan\tau_1 = \frac{\delta}{n}\tan\tau_1 \cdot \frac{d\theta_1}{\theta_1 - \theta_2},$$

$$-\frac{d_r n}{n} = \frac{1}{\eta}\left[\frac{\delta}{n}(2 - \sec\tau_1\sec\tau_2)\cot\frac{\tau_1 - \tau_2}{2} + \lambda\right]\frac{d\theta_1}{\theta_1 - \theta_2}.$$

Including also change $d\theta_2$, the total error is

$$-\frac{dn}{n} = \frac{\delta}{n}(\tan\tau_1 d\theta_1 + \tan\tau_2 d\theta_2)/\overline{\theta_1 - \theta_2}$$
$$+\left[\frac{\delta}{n}(2 - \sec\tau_1\sec\tau_2)\cot\frac{\tau_1 - \tau_2}{2} + \lambda\right]\frac{d\theta_1 - d\theta_2}{\eta(\theta_1 - \theta_2)}.$$

Wear at S or at the pendulum contact gives $d\theta_1 = d\theta_2$ so that the resulting error will be zero if $\tan\tau_2 = -\tan\tau_1$ $(= -\tan\tau)$ or $\theta_2 = -\theta_1$ $(= -\theta)$.

This setting should therefore be adopted and the equation then takes the form

$$-\frac{dn}{n} = \left(\frac{\delta}{n}\tan\tau + \frac{2}{\eta}\cdot\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{d\theta}{2\theta} \qquad \ldots\ldots(18).$$

(b) *Intensity error.* For $d\sigma$, when $\theta_2 = -\theta_1$

$$\frac{dB}{2n^2\alpha} = -\frac{2n\delta\alpha}{2\sigma\sin\tau} \cdot \frac{2d\sigma\cos\tau}{2n^2\alpha} = -\frac{\delta}{n}\cot\tau\frac{d\sigma}{\sigma} - \frac{dn}{n}$$

$$= \left(-\frac{\delta}{n}\cot\tau + \frac{2}{\eta}\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{d\sigma}{\sigma} \qquad \ldots\ldots(19).$$

(c) *Friction error.* The direct error is zero when $\theta_2 = -\theta_1$,

and

$$-\frac{dn}{n} = -\left(\frac{2}{\eta}\cdot\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{da}{\sigma}.$$

§ 7. INTERPRETATION OF THE EQUATIONS

(a) Showed that it was desirable that the make and break positions should be symmetrical with regard to the vertical, this also makes the clock tick evenly.

$\tan\tau + (2/\eta)\cot 2\tau$ is always positive and has a minimum value $2(\eta - 1)^{\frac{1}{2}}/\eta$ when $\tan\tau = (\eta - 1)^{-\frac{1}{2}}$. Thus the limits error is never less than

$$\{2(\eta - 1)^{\frac{1}{2}} . \delta/n + \lambda\}\, d\theta/2\eta\theta.$$

(*b*) $\cot\tau - (2/\eta)\cot 2\tau$ is always positive and has minimum value $2(\eta-1)^{\frac{1}{2}}/\eta$ when $\tan\tau = (\eta-1)^{\frac{1}{2}}$. Intensity error is therefore zero for this value when $\lambda = 2(\eta-1)^{\frac{1}{2}}\delta/\eta$. This gives a minimum value of α for balancing out the error. For larger values of α suitable values of θ can be found for this purpose.

(*c*) The friction error can always be removed with a value of τ between $\pi/4$ and $\pi/2$.

For each error there is an optimum value of τ differing from that for the others. A numerical example will be useful. I designed an escapement of this type $2\frac{1}{2}$ years ago, and sought, of course, to make a, $d\sigma/\sigma$ and $d\theta$ as small as possible. To make $d\sigma/\sigma$ very small needs only moderate workmanship. I hoped that I had made $d\theta$ small with sturdy stops and stiff armature lever, and finally adopted the values $\alpha = 44'$, $\theta = 35'$ for balancing the friction error. For this amplitude $\delta/n \risingdotseq \cdot 33 \times 10^{-4}$, $\eta_1 \risingdotseq 2\cdot 1$. Recent decrement experiments show that the decay of amplitude, when the impulse lever is in action, is not more than 2 per cent. in excess of decay when the pendulum is free so that $a = 0\cdot 016\sigma$ or less. Since there is no lubrication of the steel-on-glass bearing, 0·05 seems a liberal estimate for da/a, so that da/σ is not likely to exceed 0·001. I have assumed that

$$\lambda = \alpha^2/8 = \cdot 205 \times 10^{-4},$$

in ignorance of its real value. Suppose λ were really twice as large the maximum friction error would be $-\cdot 2 \times 10^{-7}$ or 0·002 s/d.

This change in a would produce an amplitude change of $1\frac{1}{2}''$ whereas (even for constant temperature) larger variations occur. If I am right in supposing that $d\sigma/\sigma$ is really small, there is still room for improvement, to make $d\theta$ smaller.

In this clock the limits error seems to be the chief trouble.

On the assumption that $\lambda = \alpha^2/8$, the value of this error, when $\alpha = 44$, and the other values are as given above, is

$$(2 \times 0\cdot 33 \times 1\cdot 05 \times 10^{-4} + 0\cdot 205 \times 10^{-4})\, d\theta/(70 \times 2\cdot 1)$$

or $(0\cdot 69 + 0\cdot 205) \times 0\cdot 7 \times 10^{-6}$ per minute of arc. Thus the error is more than four times the circular error.

τ may be changed considerably from its optimum value without appreciable increase of this error. For the above value, which $= 0\cdot 63 \times 10^{-6}$ i.e. 0·05 s/d, the amplitude change would be $19''$. Closer working than this is possible.

Since $d\theta$ does not necessarily increase with θ, the clock should work best with small amplitude of swing and a value for θ a good deal larger than the optimum one.

To secure zero intensity error with my clock an amplitude of about $100'$ would be necessary (allowing for increased values of δ/n and η). It would not pay to balance this error, since it is not troublesome, and the larger amplitude would be bad for the limits error.

This discussion may be illustrated by a table of errors in milliseconds per day corresponding to an increase of one second of arc in amplitude for my clock working under conditions ($\alpha = 44'$, $\theta = 35'$) calculated to make the frictional error small. The circular error is given as a standard with which the escapement errors may be compared. I am now satisfied that changes in friction are very small. θ should be smaller if change in range is the main offender.

Cottingham clock E. α increases 1″

Change in impulse	Range	Intensity	Friction	Circular error
Loss of rate, ms/d	2·82	− 1·46	0·11	0·67

§ 8. GRAHAM'S ESCAPEMENT

As a further example of errors, I will give the results of an analysis of Graham's escapement, on the assumption that the live faces have been shaped so that the impulse is uniform. In addition to the main impulse the following frictional accelerations are included: $-\pi a$ on live faces, $\pm \pi b$ on dead, and $-\pi c$ due to the anchor pivot.

Thus the impulse formula is

$$2\pi\,[\sigma - a]_{\tau_1}^{\tau_2} - 2\pi\,[b]_{\tau_2}^{\pi/2} + 2\pi\,[b]_{\pi/2}^{\pi+\tau_1} - 2\pi\,[c]_{-\pi/2}^{\pi/2}$$

since the escapement operates twice per cycle.

$$B = 2\,(\sigma - a - b)\,(\cos\tau_1 - \cos\tau_2).$$

$$A\alpha = 2\,(\sigma - a)\,(\theta_2 - \theta_1) - 2b\,(2\alpha - \theta_1 - \theta_2) - 4c\alpha.$$

$$\eta\frac{d\alpha}{\alpha} = \frac{d\sigma - da}{\sigma} - \frac{2\alpha - \theta_1 - \theta_2}{\theta_2 - \theta_1}\frac{db}{\sigma} - \frac{2\alpha}{\theta_2 - \theta_1}\frac{dc}{\sigma}.$$

$$\eta_2 = \frac{2\,(b - c)\,\alpha}{\sigma\,(\theta_2 - \theta_1)}.$$

Direct errors involve a term $X = (\delta/n)\tan(\tau_1 + \tau_2)/2$ and remote errors an expression $Y = (\delta/n\eta)\,(2 + \sec\tau_1\sec\tau_2)\tan(\tau_1 + \tau_2)/2 - \lambda/\eta$, then

$$-\frac{dn}{n} = \frac{d\sigma - da}{\sigma}(X - Y) + \frac{db}{\sigma}\left(X + Y\frac{2\alpha - \theta_1 - \theta_2}{\theta_2 - \theta_1}\right) + \frac{dc}{\sigma}\,.\,Y\frac{2\alpha}{\theta_2 - \theta_1}$$

with the orthodox zero settting $X = 0$, $Y = -\lambda/\eta$. By delaying the impulse, the coefficient of $(d\sigma - da)/\sigma$ is reduced, since $Y + \lambda/\eta > X$. Without entering into details it may be said that it is not practicable to reduce this coefficient to zero. This first error arises from train friction and live pallet friction. The error is reduced by delaying the impulse. Still more is error due to anchor pivot friction reduced in this way, whilst for dead pallet friction it would be possible to retard too much.

It would appear that the orthodox setting is not the best, so far as escapement errors are concerned. It should be said, however, that by delaying the impulse the barometric error is increased. This error would be reduced by application of the impulse before the zero position has been reached, but this is not practicable.

Limits errors have not been given. Such errors, due to change of level, balance out with alternate swings. Play in the anchor lever slot which embraces the pendulum can produce pronounced changes in action when oil is present, for sometimes surface tension may maintain one-side contact throughout, whilst at other times normal rocking takes place. I advocate that the anchor should be counterpoised so that one-side contact may always be secured.

§ 9. METHODS OF CALCULATING D

In the calculation of the remote error, $(d/d\alpha)(B/\alpha)\,d\alpha$ has been calculated above as $(dB/d\alpha - B/\alpha)\,d\alpha/\alpha$. Other methods are sometimes preferable or even necessary.

(1) If an impulse lever reacts on a smooth cylindrical surface fixed to the pendulum (having its axis parallel to the axis of swing), the impulse with zero setting is $[\pi\sigma\theta]_{\theta_1}^{\theta_2}$. Here

$$B = \int_{\tau_1}^{\tau_2} \sigma\alpha \sin^2\tau\,d\tau = \frac{\sigma\alpha}{4}[2\tau - \sin 2\tau]_{\tau_1}^{\tau_2}$$

In this case it is simpler to calculate $d/d\alpha\,(B/\alpha)$.

(2) $\frac{d}{d\alpha}\int_{\tau_1}^{\tau_2} h\,d\tau$ can be calculated as $\int_{\tau_1}^{\tau_2}\frac{d\,(h\,d\tau)}{d\alpha}$. We have to compare an element for the enhanced value of α with one for the original value of α. If we choose pairs having the *same values of* τ, these will be identical, provided that h is independent of α. In this case the only change is at the limits and

$$\frac{d}{d\alpha}\int_{\tau_1}^{\tau_2} h\,d\tau = [h\,d\tau]_{\tau_1}^{\tau_2} = -\frac{1}{\alpha}[h\tan\tau]_{\tau_1}^{\tau_2}.$$

Thus in the above case we have $-\frac{\sigma\,d\alpha}{\alpha}[\sin^2\tau\tan\tau]_{\tau_1}^{\tau_2}$ at once.

(3) In the Shortt clock, inertia of the impulse lever plays an important part, so that the impulse is a somewhat complicated function, not only of pendulum position but also of its velocity, and the only method I have found practicable is to calculate the impulse for a series of values of θ.

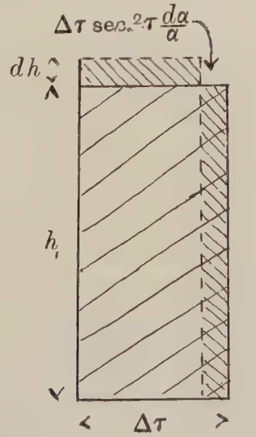

Fig. 5. Showing $d\,(h\Delta\tau)$.

In such a case it is necessary to compare two elements $h\Delta\tau$ for the *same values of* θ. Here $\Delta\tau$ changes as well as h (Fig. 5). Thus, τ changes to $\tau - \tan\tau\,(d\alpha/\alpha)$: hence $\Delta\tau$ becomes $\Delta\tau - \Delta\tau\sec^2\tau\,(d\alpha/\alpha)$. Then

$$\frac{d}{d\alpha}\int_{\tau_1}^{\tau_2} h\,d\tau = \int_{\tau_1}^{\tau_2}\left(\frac{dh}{d\alpha}-\frac{h\sec^2\tau}{\alpha}\right)d\tau.$$

It will be noticed that, if $h = f(\tau)$,

$$\frac{dh}{d\alpha} = -\frac{dh}{d\tau}\frac{\tan\tau}{\alpha},$$

and then the integral becomes

$$-\frac{d\alpha}{\alpha}\int_{\tau_1}^{\tau_2} d(h\tan\tau) = -[h\tan\tau]_{\tau_1}^{\tau_2}\frac{d\alpha}{\alpha}$$

as before.

§ 10. SHORTT CLOCK

Of escapements which I have attempted to analyse, the Shortt is the most interesting and baffling. I sought to account for errors by assuming that they were due alone to friction in the lever and wheel pivots, but found that no reasonable change in these would account for the variations in amplitude observed by Sampson* in the famous "SH_0." The average moment at which the impulse starts is slightly affected by the degree of control called for by the slave pendulum, but this again fails to account for results. Possibly there may have been small changes in the position of the lever pivot in relation to the pendulum bracket.

The foregoing method for ascertaining errors is applicable to all types of drive, but results are reliable, of course, only in so far as data are adequate. Some clocks have scape wheels so massive that the neglect of inertia seems scarcely permissible. In the Cottingham clock, inertia of the impulse lever is most significant at the moment of collision with the pendulum, but even here the effect appears to be unimportant.

In all escapements reacting on the pendulum near the top, the suspension spring is deflected. No account of this deflection has been taken in this paper, but I am sure that its effect is not negligible.

DISCUSSION

Mr Rollo Appleyard referred to an early device due to Sir Francis Ronald for overcoming coherence in the electric pendulums employed at the beginning of the nineteenth century. The cure was to stretch a steel wire across the path of the flexible pendulum rod. This produced a flick which overcame the sticktion at contact.

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* *Proc. R.S. E.* **44**, 71 (1924).

Mr F. Hope-Jones. I am surprised that Mr Atkinson has had the temerity to invite me to hear his paper on escapement errors and to take part in the discussion upon it when only a few years ago I said that—so far as clocks of precision were concerned—escapements were dead, killed by the free pendulum, just as the art of steel engraving had been killed by photogravure. Until a few years ago, clocks of precision were defined as those attempting to measure time with an accuracy of one part in a million: they were then lifted to an altogether higher plane, with an accuracy nearer to one part in 100 millions.

Mr Atkinson's equations will give the layman a good idea of the difficulty of time measurement of that order. It has now been proved that it cannot be done unless all these complex considerations—barometric errors, escapement errors, and all the ills that clocks are heirs to—are eliminated by putting the pendulum *in vacuo*, and leaving it alone, except for its impulse once every half-minute. If this impulse, beginning from zero when it is assisting gravity, could finish equally after zero when it diminishes gravity, then there would be no escapement error.

The Cottingham clock which Prof. Sampson found so useful for his mathematical analysis of escapement errors is Froment, 1854. Its fault is common to all gravity escapements, that of using the pendulum to raise a lever at the end of its swing and allowing the lever to return with it to a lower position, the difference being the impulse. Why trouble to make a mathematical analysis of an escapement which is so fundamentally wrong as to subject its pendulum to constant interference at the end of each swing, when it is doing most of its job of time measurement and where it is most sensitive to disturbances of any kind?

With regard to the pivot friction in the Shortt clock, I understood Mr Atkinson to suggest, as many have suggested, that since the impulse must be 30 times normal when delivered only once every half-minute instead of every second, the pivot friction of the gravity lever and variations thereof must also be 30 times normal and that you are no better off. But surely the factor that matters is the time during which such disturbances are free to act on the pendulum. The total period during which a pendulum is subject to interference should always be compared with time itself, that is to say, the total time measured. A pendulum is only safe from interference when it is absolutely free. It would be instructive to review all known escapements from that standpoint and to place them in an order of merit based upon the degree of their freedom. Such a list would begin with Harrison's Grasshopper, in which the escapement is at work all the time and the pendulum is never free, and would end with the Shortt free pendulum in which the interference is confined to one part in one hundred of the time measured. But there is a practical consideration of overwhelming importance. The total energy which has to be imparted to a free pendulum *in vacuo* is that absorbed by the flexure of the spring, and it is only 0·08 gm. cm. per half-minute, or 0·4 gm. falling 2 mm. If you attempt to divide that into 30 impulses your gravity lever must not weigh more than one fifteen-hundredth part of an ounce, and must not fall more than one-eighth of an inch. That is impracticable; but even if you could do it the ratio of pivot friction (and of course variation of pivot friction) would be enormously increased. In reducing the size of the impulse, you cannot cut down the friction in the same proportion. And it would be useless as a switch; even in its half-minute form it was insufficient to serve my purpose, hence the origin of its use to release the heavier lever of a Synchronome switch. This dissociation of the two functions and the provision of a separate lever for each was designed by Mr Shortt, and contained merits beyond those contemplated at the time.

AUTHOR'S REPLY. Although the errors of the Shortt clock are theoretically the same as if the impulse were subdivided, I admit that it would be inexpedient to do this in practice. I agree that the friction would thereby be increased. When Mr Hope-Jones says that "escapements" are dead he uses the word in a more restricted sense than I intend. I believe that I have conformed to custom, but this is perhaps misleading and the word seems to suggest toothed wheels.

I think that the Cottingham clock is a good illustration of the application of the theory, just because, as Mr Hope-Jones says, the impulse acts at the end of the swing and so fails to conform to Airy's criterion. Analysis shows that it is not necessarily bad on this account. Prof. David Robertson* would explain this by saying that the lever and pendulum form one piece during the impulse, whilst interference is confined to the moments when the lever is picked up and dropped. The lever friction must, however, not be omitted in computing the errors.

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* *Horological Journal*, **71**, 272 (1929).

APPARATUS FOR DETERMINING THE SPECIFIC HEAT OF A MATERIAL IN POWDER FORM

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ABSTRACT. A calorimeter suitable for heavy powdered materials, such as dry clay, is described. It utilises the electrical method, and embodies a special form of stirrer suitable for these materials.

§ 1. INTRODUCTION

Unless a material is either liquid or a good conductor of heat, the determination of its specific heat presents considerable difficulty. The methods ordinarily employed fall into two classes, the method of mixtures and the electrical method. In the former, the body is heated and transferred to a calorimeter containing a liquid of known specific heat. The quantity of energy involved in changing its temperature is estimated from the rise in temperature of the calorimetric fluid, when equilibrium is attained. In the electrical method, energy is generated electrically, its quantity being measured, and the apparatus is so arranged that the energy all passes into the body under examination; the specific heat is deduced from the rise of temperature.

In either method it is essential that initially and finally the body shall be in a state of uniform temperature, and when the body is a poor heat conductor the equalisation of temperature may take a considerable time. During this period, of course, the body or the calorimeter is losing heat at a rate depending on its temperature excess above its surroundings. It is usual to correct for the error thus introduced, either by raising the temperature of the surroundings so that this excess is always zero, or else by estimating the heat loss as a function of temperature excess, by separate experiments. The importance of this correction increases rapidly when the correction itself becomes an appreciable fraction of the total heat interchange, as it necessarily does when the time for equalisation of temperature is prolonged.

§ 2. DESCRIPTION OF APPARATUS

The apparatus to be described is suitable for powdered materials. It utilises the electrical method, and is designed so that uniformity of temperature in the material may be obtained by thorough mixing with a special form of stirrer capable of dealing with powders.

An axle passes down the centre of the calorimeter, and blades of helicoidal form are attached to this axis by a stirrup free to swing about a horizontal axis. Hence, as the stirrer is rotated it picks up the material and throws it over, somewhat after the manner of a ploughshare, see Fig. 1. The stirrer is carried on ball-bearings so as to minimise heat production by friction, and by means of a wooden platform attached to the axis is slowly rotated. This platform is utilised also as a stand for a vacuum flask. The mean temperature of the material is obtained by means of a number of thermo-couples in series, of which the junctions are attached to the blades of the stirrer, but insulated from it by means of very small glass tubes. The thermo-couples thus rotate with the stirrer; by the bringing of the cold junctions to the vacuum flask mentioned above, the need for elaborate contacts between rotating and stationary parts is avoided. When temperature readings are required, contact is made to the instrument (e.g. a potentiometer) used for measuring the e.m.f. by the slipping of two knife-blade contacts into the switch parts attached to the platform as shown.

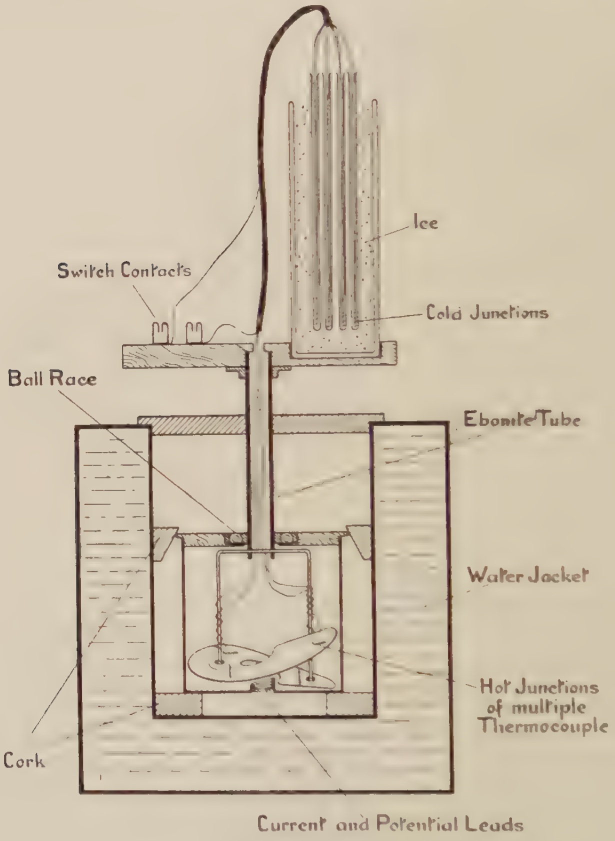

Fig. 1.

Heat is generated in the interior of the mass of material by means of the heater seen just below the stirrer in Fig. 1. It is located in a re-entrant tube so that all the heat generated reaches the material under examination. Potential leads are taken from the coil at the point of emergence from the tube, so that heat generated in the leads is not included in the quantity measured.

To minimise the heat loss from the calorimeter, the latter is closed by a wooden lid, with metal foil on the under surface to act as a radiation shield. The calorimeter is placed upon a slab of cork inside a larger double-walled vessel used as a constant-temperature water jacket. The calorimeter is prevented from rotating by two wedge-shaped pieces of cork bearing against the projecting edge of the calorimeter lid; the water jacket is itself closed by an outer cork lid. The thermal capacity of the apparatus is determined by means of a blank experiment, in which a measured amount of heat is generated with water as calorimetric fluid, and the correction for radiation losses is obtained by observation of the rate of cooling with the energy switched off. In the apparatus as we have constructed it, the water equivalent of the calorimeter and accessories is 74 gm. and it holds 800 gm. of material of density about 2 and specific heat 0·2. Successive tests with different weights of a given material give results in excellent agreement, and it has also been verified that the temperature-rise, after correction for losses, is unaltered if the energy supply be cut down so as to occupy three times as long in importing the same amount of energy.

It may be pointed out that the kindred apparatus previously described by one of us* is primarily intended for light powdered materials, such as cork dust, and that the apparatus now described is not intended to take its place, but was designed, and has been found satisfactory, for heavier powdered materials, such as dry clay.

§ 3. ACKNOWLEDGMENT

We wish to express our thanks to Mr A. R. Challoner, observer in the Physics Department, for assistance both in the design and in the construction of the apparatus.

DISCUSSION

Dr Allan Ferguson. It would be useful if Mr Awbery would give us some information concerning the accuracy aimed at in these interesting experiments. Principal E. H. Griffiths, in his specific heat work, advocated such vigorous stirring that the stirring itself caused a measurable rise of temperature which was duly allowed for. Does Mr Awbery advocate agitation of this vigorous type in dealing with powders, or is he able to obtain consistent and accurate results with more gentle stirring?

Prof. F. L. Hopwood enquired how fine were the powders investigated. Very fine powder, like flour, would be difficult to stir.

The President enquired whether the readings of the thermo-couples employed differed to an extent which might indicate inadequate stirring.

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* *Proc. Phys. Soc.* **33**, 355 (1921).

Author's reply. The apparatus is only designed for an accuracy of about 2%, speed of operation being an element of the design. The procedure advocated by Principal Griffiths is, however, desirable when liquids are to be used and the highest accuracy is desired. In reply to Prof. Hopwood: the apparatus would be capable of dealing with flour in a dry condition, although actually it was used for rather coarser materials. The point raised by the President is an important one, which we had not overlooked; a special examination showed that the agreement between the individual thermo-couples was very satisfactory.

HEAVISIDE'S OPERATIONAL METHOD

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ABSTRACT. The author describes a method of treatment of Heaviside's operational calculus which involves unrestricted use of complex integration as employed by Bromwich, and concludes that the theory of integration provides a complete explanation of all the aspects of Heaviside's method of solving differential equations.

§ 1. INTRODUCTION

IT is proposed to show that it is possible to obtain a complete explanation of Heaviside's method of solving differential equations, by a method which incorporates the views of the late T. J. I'A. Bromwich with those of J. R. Carson. The author's method is, to the best of his belief, identical with that advocated by Van der Pol*. In contrast with the procedure recently recommended by W. E. Sumpner† at a meeting of the Physical Society, it avoids the uncertainties that arise from the use of 'operators.' For compactness of exposition, the fundamental definitions and theorems will first be stated.

§ 2. DEFINITIONS AND THEOREMS

When the function $F(p)$ of the complex variable p is derived from a function $f(t)$ of the real positive variable t (where $f(t)$ is zero for negative values of t) by the transformation

$$F(p) = \int_0^\infty e^{-pt} f(t)\, dt \qquad \ldots\ldots(1),$$

the function $F(p)$ is called the *convert* of $f(t)$, and the process whereby $F(p)$ is obtained from $f(t)$, *conversion*. In general, conversion will be indicated by changing the small letter denoting a function of t to the corresponding capital letter, which then denotes a function of p. An important property of converts is enunciated by Borel's‡ theorem. Let $f(t)$ be connected with $f_1(t)$ and $f_2(t)$ by the relation

$$f(t) = \int_0^t f_1(t-\tau) f_2(\tau)\, d\tau \qquad \ldots\ldots(2).$$

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* "A Simple Proof and an Extension of Heaviside's Operational Calculus for Invariable Systems," *Phil. Mag.*, **7**, 1153, (1929).

† "Heaviside's Fractional Differentiator," *Proc. Phys. Soc.*, **41**, 404 (1929).

‡ Borel, *Leçons sur les Séries Divergentes*, p. 104 (1901).

Then if $F(p)$, $F_1(p)$ and $F_2(p)$ be the converts respectively of $f(t)$, $f_1(t)$ and $f_2(t)$, Borel's theorem states that

$$F(p) = F_1(p)\, F_2(p) \qquad \text{......(3).}$$

When a function $f(t)$ of the real positive variable t is derived from a function $F(p)$ of the complex variable p by the transformation

$$f(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p)\, e^{pt}\, dp \qquad \text{......(4),}$$

the function $f(t)$ is called the *revert* of $F(p)$, and the process whereby $f(t)$ is obtained from $F(p)$, *reversion*. Reversion is thus defined by an integral of the type used by Bromwich. The integration is along a straight line, the constant c is positive, and arbitrary so long as all the singularities of $F(p)$ are to the left of the line of integration.

With this nomenclature, the statement of the celebrated Mellin-Fourier theorem* (already implied by the notation) is that, with suitable restrictions on the function concerned, the revert of the convert of $f(t)$ is the function $f(t)$ itself, or in terms of integrals, if

$$F(p) = \int_0^\infty e^{-p\tau} f(\tau)\, d\tau,$$

then

$$f(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p)\, e^{pt}\, dp \qquad \text{......(5).}$$

In the application of the theorems to the solution of differential equations, it has further to be noticed that the convert $F_n(p)$, of the nth differential coefficient, $f_n(t)$, of $f(t)$ is obtained by integration by parts, thus

$$F_n(p) = p^n F(p) - p^{n-1} f(0) - p^{n-2} f_1(0) - \dots - p f_{n-2}(0) - f_{n-1}(0) \qquad \text{......(6).}$$

Finally, when $f(t)$ involves other variables than t the convert of a differential coefficient with respect to one of these other variables is to be obtained by the rule of differentiation under the sign of integration. Thus the convert of df/dx is, in general, dF/dx.

§3. COMPARISON WITH HEAVISIDE'S CONVENTIONS

Heaviside's operational method for the solution of differential equations consists, essentially, of three steps: (i) transformation to the operational form; (ii) solution of the operational equation; (iii) "algebrisation" of the operational form of the solution. In the present method, these three steps are represented by (i) conversion; (ii) solution of the converted equation; (iii) reversion.

The definitions used in the present method involve a notation slightly different from that of Heaviside, who considers, for instance, p^{-n} as operating on the unit function of t (which is equal to unity when $t > 0$, and zero when $t < 0$). Thus he writes

$$p^{-n} = t^n/\Gamma(n+1).$$

In the notation proposed here, p^{-n} is the convert of $t^{n-1}/\Gamma(n)$, and such an expression as $p^{-n}\, t^m$ is meaningless.

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* Bromwich, "Normal Coordinates in Dynamical Systems," *Proc. L.M.S.* 2 15, 412 (1916).

The key to the correctness of Heaviside's method appears to be undoubtedly the Mellin-Fourier theorem, equation (5), which shows that if the required solution of a differential equation be sufficiently regular in its behaviour, then the Bromwich-Heaviside method will, certainly, find this solution, and no other.

To illustrate this comparison by a well-known particular case, consider the equation of diffusion

$$\frac{d^2y}{dx^2}=\frac{dy}{dt}$$

where $y=0$ when $t=0$, $y=y_0$ when $x=0$, and $y\to 0$ when $x\to\infty$. Here y_0 may, of course, be a function of t.

Heaviside's method consists in writing p for $\frac{d}{dt}$ so that

$$\frac{d^2y}{dx^2}=py$$

and accordingly $$y=y_0\exp\left(-p^{\frac{1}{2}}x\right).$$

This equation is then interpreted as defining y as a function of the two variables x and t by Heaviside's rules of "algebrization."

In the present method Y and Y_0 are supposed to be the converts respectively of y and y_0 so that, by (6) the equation for Y is

$$\frac{d^2Y}{dx^2}=pY$$

with the condition $Y=Y_0$ when $x=0$.

Thus $$Y=Y_0\exp\left(-p^{\frac{1}{2}}x\right).$$

Then, since by definition Y is the convert of y, the solution of the equation is given by an integral of Bromwich's type.

$$y=\frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty}Y_0\exp\left(-p^{\frac{1}{2}}x+pt\right)dp.$$

To interpret Heaviside's method it is only necessary to consider the function dealt with in the operational form of the equation to be the convert of that in the original equation. When the operational form of the equation has been solved, a final reversion completes the explicit solution of the original equation.

§ 4. EXAMPLES

On p. 156 of Carson's book* two integral equations are quoted, Laplace's equation

$$F(p)=\int_0^\infty e^{-pt}f(t)\,dt,$$

where $f(t)$ is the function to be found, and Poisson's equation

$$\phi(t)=f(t)+\int_0^t\phi(\tau)\,k(t-\tau)\,d\tau,$$

where $\phi(t)$ is the function to be found.

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* *Electric Circuit Theory and the Operational Calculus* (1926).

Concerning Laplace's equation, Carson says, "Little work has been done on Laplace's equation from the standpoint of pure analysis." The general solution of this equation is, however, known, for the function $f(t)$ is found by reversion (4) according to the Mellin-Fourier theorem (5). When, owing to peculiarities in the structure of $F(p)$, this solution is inapplicable, integrals of $f(t)$ may be found by reverting $p^{-1} F(p)$ or $G(p) F(p)$, where $G(p)$ is suitably chosen. The discussion of these cases would follow the orthodox lines. From the physical point of view, the theory of this equation may be regarded as complete; the difficulties are connected only with numerical work and analytical abnormalities.

Poisson's equation is solved by conversion, which gives, by Borel's theorem (3), and in accordance with our conventions

$$\Phi(p) = F(p) + \Phi(p) K(p),$$

whence

$$\Phi(p) = F(p)/\{1 - K(p)\},$$

and the solution is, accordingly

$$\phi(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \frac{F(p)\, e^{pt}\, dp}{1 - K(p)}$$

when the integrand behaves suitably.

The solutions of these equations as complex integrals may be regarded as the most satisfactory theoretical ones, as complex integrals can generally be more easily and variously manipulated than any other mathematical form. Poisson's equation when modified by the introduction of differentiations is, of course, amenable to solution by the same methods. These remarks are only supplementary to the general subject of Heaviside's method; for equations of Poisson's type are frequently solved incidentally when the effects of terminal conditions (such as networks attached to the sending or receiving end of a cable) are found without the corresponding integral equations being explicitly stated.

As a final example, consider the equation

$$\frac{d^2y}{dx^2} = \frac{d^2y}{dt^2} + \frac{2}{\Gamma(a)}\frac{d}{dt}\int_0^t (t-\tau)^{a-1} y(\tau)\, d\tau + \frac{1}{\Gamma(2a)}\int_0^t (t-\tau)^{2a-1} y(\tau)\, d\tau,$$

where $y = 0$ and $dy/dt = 0$ when $t = 0$ and a is positive.

Then, by (3) and (6), conversion gives

$$d^2 Y/dx^2 = (p + p^{-a})^2 Y.$$

If also $y = f(t)$ when $x = 0$, and $y \to 0$ when $x \to 0$

$$Y = F(p) \exp\{-(p + p^{-a})x\},$$

and the solution of the equation under the prescribed conditions is by (5)

$$y = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \exp\{-p^{-a}x + (t-x)p\} F(p)\, dp$$

$$= \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p) \exp\{(t-x)p\}\, dp$$

$$+ \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} [\exp\{-p^{-a}x\} - 1] F(p) \exp\{p(t-x)\}\, dp.$$

Now write $$\xi(x, t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \{\exp(-p^{-a}x) - 1\}.\exp pt\, dp,$$

and then owing to the behaviour of the integrand at infinity the contour of integration can, when $t > 0$, be modified to be a loop about the negative half of the real axis, and when $t < 0$, $\xi(x, t) = 0$, thus

$$\xi(x, t) = \frac{1}{2\pi i}\int_{-\infty}^{0+} \exp(-p^{-a}x + pt)\, dp,$$

and the solution of the equation is

$$y = f(t - x) + \int_{x}^{t} \xi(x, t - \tau) f(\tau - x)\, d\tau$$

when $t > x$, and $y = 0$ when $t < x$.

The value of $\xi(x, t)$ is easily found by expansion in ascending powers of x to be given by

$$t\xi(x, t) = \sum_{1}^{\infty} \frac{(-1)^{\nu}(xt^{a})^{\nu}}{\nu!\,\Gamma(a\nu)}.$$

These examples, and the theorems upon which the method of solving them is based, seem to show that avoidance of the use of complex integration involves assumptions comparable in magnitude with such theorems as the Mellin-Fourier theorem, and condemns the operational method to remaining permanently in an experimental state. The use of complex integration, does, on the contrary, encourage the belief that much of Heaviside's technique can be regarded as a consolidated gain to exact and methodical science.

DISCUSSION

Dr W. E. Sumpner (communicated). The author has given an admirably clear and concise exposition of the connexion between the two methods of Bromwich and of Carson. I desire to offer a tribute to the merits of the paper, but I do not consider that it contains anything to justify what appears to be the author's conclusion, indicated in the last paragraph, that operator methods must remain in an experimental state until justified by analysis such as that of Bromwich. My recent paper is mentioned and is apparently singled out for criticism from this point of view. It may be well to point out that it did not suggest any new method, or criticise any old one. Its aim was to offer, and to justify, a new interpretation of the operator used in Heaviside's method. The interpretation seems to be applicable to other methods, including those of Bromwich and of Carson. These methods establish, by a searching form of analysis, the equivalence of two functions of a quantity p,

$$f_1(p) = f_2(p).$$

The equivalence is much more than a numerical equality holding good for any number p whether simple or complex. It is proved true for any quantity p which satisfies the laws of algebraic combination, so that it is more correctly described by the word identity. This being so, it can be used for any operator p which conforms to such laws, so that if we denote the operand by F we have

$$f_1(p)\,F = f_2(p)\,F.$$

If now $f_1(p)$ take a form like $(p-c)^{\frac{1}{2}}$ which is not directly workable as an operator, while $f_2(p)$ can be readily interpreted as such, it is legitimate to use the second form in place of the first.

The assumptions made are two in number; p must satisfy the ordinary algebraic laws in the analysis used for establishing the equality, and it must be used in a way consistent with this assumption when the result of the analysis is being interpreted.

Now so far as the result is concerned it does not matter whether p is considered to be Boole's differentiator, or what I have called the index operator defined by the equation

$$p^{\alpha}\,t^{\beta}/\beta! = t^{\beta-\alpha}/(\beta-\alpha)!$$

since, if we take a typical result such as

$$p^2 t + p^{-3} t^2 + p t^{-1},$$

it will readily be seen that in the two cases we arrive at the same expression in terms of t.

But if we want to prove that powers of p form commutative operators in association with any operand, we find the proof quite easy when p is the index operator, but impossible if p is Boole's differentiator. It has frequently been pointed out in papers dealing with Heaviside's methods that p and p^{-1} are not commutative. If we consider the quantities

$$p^2\,.\,(p^{-3}t),\ p^{-1}\,.\,t,\ \text{and}\ p^{-3}\,.\,(p^2 t),$$

the first two become $t^2/2!$, while the last becomes zero if, as is always the practice in operator methods, arbitrary constants of integration are omitted. If p is the index operator all three quantities become $t^2/2!$.

This criticism raises no question about the accuracy of the analysis involved in the methods of Bromwich and of Carson, but it does affect the interpretation of the corresponding theorems when used with operators. The use of the index operator with Heaviside's methods does not affect these methods, but it removes much of the obscurity associated with them. In particular it makes it needless to explain the meaning of $p^{\frac{1}{2}}$ in terms of the calculus. The operator is so defined as to interpret $p^{\frac{1}{2}}$ without reference to the calculus, but on the other hand the use of the operator has to be justified. My paper did this for certain simple cases. I have since found that it does so in more complex examples.

I differ from the view taken by the author. I consider that Heaviside's method can be justified without any reliance either on the use of the calculus or on the use of complex integration.

The PRESIDENT thanked the author for his concise exposition. In the past the speaker had had occasion to work a good deal with Heaviside's method and he had been obliged to read some very laborious justifications of it, occupying in some cases twenty or thirty pages. The proof given by the author, on the other hand, was a model of conciseness.

Author's reply. The definition of p as an index operator, as recommended by Dr Sumpner, seems to depend upon the expansion of the operand in a power series, but this restriction is unnecessary when p is introduced by conversion, as advocated in the paper. The demonstration of the properties of the index operator by examination of particular cases does not appear to offer complete assurance that other important particular cases may not evade verification.

In my opinion Dr Sumpner exaggerates the significance of the fact that a function of p can be expressed in two ways, and my view may be illustrated by an example of the type quoted recently by Bromwich from p. 244 of the second volume of *Electromagnetic Theory*. Consider, for instance, a function $F(p)$ which is expansible in a series of descending powers of p for large values of p, and in a convergent series of ascending powers of p for small values of p. Then the index rules applied to the interpretation of $F(p)$ operating on the unit impulse function $H(t)$ give two series, one of which is an everywhere convergent series of ascending powers of t, and the other an asymptotic series of descending powers of t, which does not always fully represent the correct result. As Heaviside says, in p. 488 of the same volume, "it usually happens that the divergent result is not the full equivalent of the convergent result." We may take, as a particular example,

$$F(p) = p^{3/2}/(1 + p^2),$$

where, accordingly,

$$F(p) = p^{-1/2} - p^{-5/2} + p^{-9/2} + \dots$$

when $|p| > 1$, and also

$$F(p) = p^{3/2} - p^{7/2} + p^{11/2} + \dots$$

when $|p| < 1$. It is to be noticed that the two series represent precisely the same function, but in different regions; while they are certainly not operationally equivalent, when p is regarded as an index operator. The operational identity may be written in the paradoxical form,

$$\frac{p^{3/2}}{1+p^2} - \frac{1}{\sqrt{2}} + \frac{1}{\sqrt{2}} \cdot \frac{1-p}{1+p^2} = \frac{p^{3/2}}{1+p^2},$$

where the left-hand side must be expanded in descending powers of p, the right-hand side in ascending powers of p, and the operand is $H(t)$. The general rule, of course, for constructing such cases is to write down a function $F(p)$ which has both branch-points and poles. In the cases usually considered there is one branch-point at $p = 0$ and a pair of poles. The residues at the poles are responsible for the discrepancy between the asymptotic and convergent expansions obtained by the index rules. When complex integrals are used the question of commutation of operations does not arise, and I am inclined generally to regard the operational rules merely as a technical convenience. Finally, I thank both the President and Dr Sumpner for their courteous remarks.

THE J-PHENOMENON IN X-RAYS

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Communicated by Prof. P. W. Burbidge, October 19, 1929. Read December 13, 1929.

ABSTRACT. A series of experiments have been carried out with the view of repeating as far as possible the work of Barkla and others on the J-phenomenon. The results obtained in no case provide any evidence for this phenomenon, and, considered in conjunction with the work of Dunbar, Worsnop, and Gaertner, the experimental results suggest that the J-phenomenon has no real existence as an X-ray absorption effect.

§ 1. INTRODUCTION

In a number of papers, Barkla and co-workers have described an effect which was observed in experiments on X-radiation; this was at first considered to be due to a characteristic radiation of short wave-length, but is now attributed to a new absorption phenomenon—the so-called J-phenomenon.

The evidence for and against the existence of this characteristic radiation may be considered briefly. Barkla and others found:

(*a*) Scattered heterogeneous radiation to be softer than the primary radiation, the amount of softening varying with the primary wave-length, as would be expected were a characteristic radiation excited(1).

(*b*) Increased corpuscular radiation from aluminium at short wave-lengths, associated with J-absorption(1).

(*c*) Discontinuities in absorption in aluminium, carbon, oxygen, etc.(2).

These discontinuities were explained as a result of J-absorption. Williams(3), using a monochromatic radiation, found similar discontinuities for aluminium and copper. In many cases, however, his results appear to be open to a graphical interpretation other than that given, nor are they particularly concordant with regard to wave-length. Owen(4) found slight selective absorption of a narrow band of wave-lengths and, as a tentative suggestion, ascribes the results to J-absorption in silicon in the crystal used. His results have not been confirmed. Dauvillier(5) reported absorption-discontinuities for aluminium, and Khastgir and Watson(6) claimed spectroscopic evidence for a J-radiation.

The investigations of Richtmeyer(7), Hewlett(8), Hull and Rice(9), Olson, Dershem, and Storch(10), Allen(11) and Dunbar(12), have failed to show absorption-discontinuities in aluminium, copper, molybdenum, silver, water, carbon, oxygen, lithium, iron, and sulphur, at the predicted wave-lengths. Most of these observers employed spectrometric methods and used monochromatic radiation; that employed by Dunbar was heterogeneous. Richtmeyer also explains certain of Dauvillier's results. No trace of J-emission was found by Dauvillier for boron, or by

Duane and Shimizu(13) for aluminium. Dunbar explains the excess corpuscular radiation at short wave-lengths on Compton's theory. Siegbahn(14) and Nipper(15) both show that no spectroscopic evidence such as that claimed by Khastgir and Watson exists.

The weight of evidence is thus against the existence of a J-characteristic radiation, and Barkla now considers(16) that no direct evidence for such radiation exists.

§ 2. BARKLA'S LATER THEORY

Since the hypothesis of a new characteristic radiation could not be entertained, Barkla found it necessary, in order to explain experimental results, to postulate a new phenomenon in X-ray absorption. This phenomenon he has stated briefly thus: "A beam of X-rays in transmission through matter under certain critical conditions becomes considerably more absorbable both in that and in other substances"(17). "The transformation is not in the process of scattering, but in the transmission of scattered radiation through the radiating substance and through absorbers"(17). "The phenomenon is, however, neither X-ray scattering nor X-ray fluorescence, as these terms have been understood"(16). "After the J-transformation has taken place in a scattered X-radiation, and this has become a 'modified scattered' radiation when its absorbability is measured in certain substances, it has, even after passing through these substances, precisely the same absorbability as the primary when measured in certain other substances"(18).

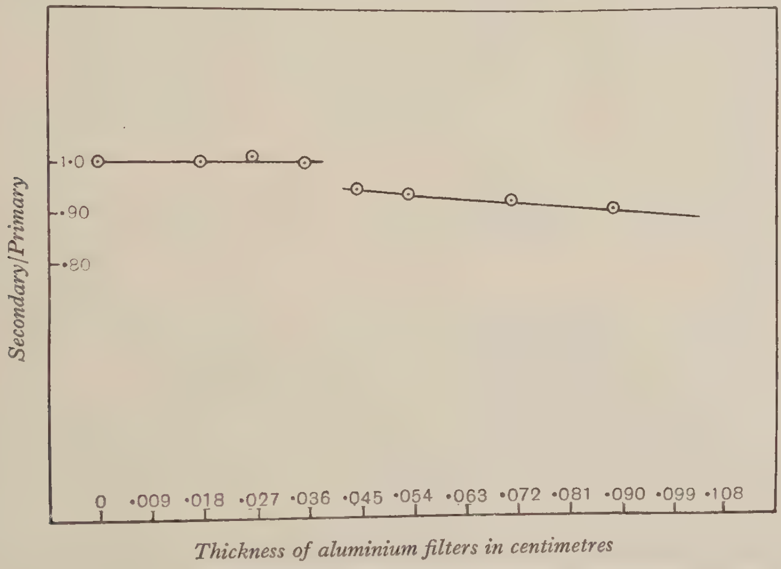
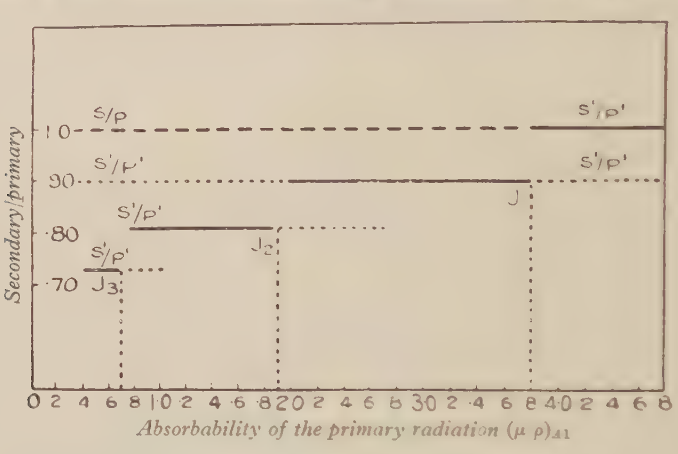


Fig. 1. Secondary and primary beams filtered simultaneously through aluminium sheets.

Soft tube: $\left(\frac{\mu}{\rho}\right)_{AL}=3\cdot5$.

This phenomenon comprises, in certain experiments, a softening of the scattered radiation and is thus similar to the effect explained on Compton's theory. In the experiments on the J-phenomenon, however, Barkla finds that in no case does Compton's theory of scattering apply.

The experimental results may now be considered. Barkla and Mrs Sale(19) found in general the hardness of primary and scattered radiations to be identical, though at times the scattered radiation appeared softer. The latter was not, however, usually the case. Barkla and Khastgir(20) determined the change in ratio of intensities of scattered and primary radiations (as measured by their ionising-action) on absorption of each by equal thicknesses of aluminium. Discontinuities are observed and, as will be seen from Fig. 1, they occur in every case in the scattered radiation. These results are taken to show that the transformation into a softer radiation takes place outside the scattering substance. In 1925 Barkla(16) pointed out additional characteristics of the phenomenon. The discontinuity is absolutely abrupt, even with heterogeneous beams, and appears to be more closely connected with absorption coefficient than with wave-length. The scattered radiation appears much more susceptible to transformation than the primary, and the appearance or non-appearance of the effect seems to be governed by unknown conditions. Barkla and Khastgir(20) observed discontinuities in absorption of scattered radiation. The ratio of the intensities of secondary and primary radiations, after absorption in equal thicknesses of aluminium, was found to remain constant, as the primary radiation was hardened, up to a certain critical point, when the ratio changed abruptly to another value, which again remained constant over a certain range. As many as five separate discontinuities of this nature have been observed.



Broken line - - - - - - - is "unintercepted ratio" S/P.
Continuous line ——— is "intercepted ratio" S′/P′.
Dotted line . . . . is occasional "intercepted ratio" S′/P′.

Fig. 2.

Fig. 2 shows the nature of this effect. The primary radiation could be hardened either by filtering or by increasing the tube potential, but in each case the discontinuity appeared at the appropriate absorption coefficient. Similar experiments were described in November(21) 1925, when Barkla and Khastgir point out this feature: If S and P be the intensities of scattered and primary radiations, and S' and P' the intensities after absorption in equal thin sheets of aluminium, the ratio $S/P : S'/P'$, that is $e^{-(\mu_1-\mu_2)x}$ where μ_1, μ_2 are the absorption coefficients of primary and scattered radiations, and x the thickness of absorber, is found to be accurately constant, and independent of wave-length.

Barkla and Miss Mackenzie (22) observed discontinuities in absorption of radiation scattered at an angle of 120° to the primary beam. Other experiments gave, instead of discontinuities, a continuous variation in the ratio of ionisations produced by beams scattered at 60° and 120°. Results are also quoted to show that the absorption of a complex beam is not the sum of the absorptions of its constituents. This is deduced from the fact that on superposing two radiations, each showing a J-discontinuity at a particular thickness of absorbing screen, instead of two discontinuities being observed, only one appears, and this at a thickness of absorber appropriate to the new complex radiation. Barkla and Khastgir (23) showed that a radiation which appeared to be modified when its absorption was measured in certain substances, appeared to be unmodified in others. Thus the J-phenomenon appears to depend in part on the material of the absorbing screen. Barkla and Miss Mackenzie (24) found the J-phenomenon to be dependent on the frequency of interruption of the primary current in the induction coil used to excite the X-radiation. The J-discontinuities in absorption appeared only over a limited range of frequencies, the critical frequency being dependent on the particular X-ray bulb employed.

Barkla and Watson (25), using a Coolidge tube, found that the position of the discontinuity, i.e. the absorption coefficient at which it occurred, depends on the current through the tube. They suggest that a possible explanation of this fact is to be found in the method used to measure the absorption coefficient. They also suggest that in cases where a continuous difference in absorbability of two radiations is found an explanation is to be sought in the fact in question.

Barkla (26) found that, with increasing thickness of scattering material, the scattered radiation became increasingly softened, the amount of softening ultimately reaching a limiting value. In other cases, however, with apparently the same experimental arrangement, the full amount of softening was produced by the thinnest sheets.

The chief experimental results summarised from the various papers appear to be as follows:

1. Softening of scattered radiation, such as would be expected were a characteristic radiation excited (1).
2. Increased corpuscular radiation at short wave-lengths (1).
3. Absorption discontinuities (2, 3, 4, 5).
4. Equality of hardness of scattered and primary radiation as a general rule, though at times a softening of the secondary radiation occurs (19).
5. Discontinuities in absorption of scattered radiation, the discontinuities appearing at definite values of absorption coefficient, independent of the method of attaining this hardness (20, 21, 22, 23, 24, 25).

6. Change of absorption coefficient at the discontinuity independent of wave-length(21).

7. Discontinuities in radiation scattered at an angle of 120°, with occasionally a continuous softening of this beam as compared with one scattered at 60°(22).

8. Absorption of a complex beam not the sum of the absorptions of its constituents(22).

9. Radiation modified in some substances but unmodified in others(23).

10. Dependence of the phenomenon on frequency of exciting impulses, and on current through the X-ray bulb(24, 25).

11. Dependence of the amount of softening of a scattered radiation on the mass per unit area of scattering material(26).

In the work published on this subject, several discrepancies may be observed. It is stated(17) that the changes are absolutely abrupt, even with heterogeneous beams. This effect is not shown in earlier experiments(1), nor is it of the type expected to be produced by the excitation of a characteristic radiation by a heterogeneous primary beam. It is also suggested as possible(27) that the absorption is an absorption of all the constituents of the radiation. This does not agree with the results obtained by Owen, where selective absorption is noted. It is remarkable also that when direct radiation is compared with that scattered at 90°, it is always the scattered radiation which shows the discontinuities, never the primary beam. Similarly, on comparing the radiations scattered at 60° and at 120°, it is found that in the latter only are discontinuities observed. This suggests that the phenomenon is dependent on the angle of scattering, though apparently that inference has not been investigated.

The work of other observers has in few cases afforded evidence for the J-phenomenon. Watson(28) considered that the effect might be due to some change in the absorbing material, and proposed to test this by means of a second beam from an independent source. Results were, however, not conclusive. Crowther(29) found that radiation scattered from aluminium, paraffin wax and paper was more absorbable than the primary radiation, but since he used heterogeneous beams his results may be completely explained by the Compton effect of change of wave-length on scattering, and of variation of scattering coefficient with wave-length.

Compton(30), using a balance method, found the scattered radiation to be appreciably softer than the primary, the amount of softening agreeing closely with that predicted by the quantum theory. Beatty(31) found X-radiation scattered from air to be softened, and Sadler and Mesham(32) observed a similar effect for a homogeneous radiation scattered from carbon. Dunbar(12) repeated much of Barkla's work but found no evidence for the J-phenomenon, though certain irregularities which appeared in his results have been interpreted as such by Barkla. Dunbar found heterogeneous scattered radiation to be softer than the primary by an amount agreeing closely with the results of Compton's theory.

Towards the completion of the present work, papers by Worsnop and by Gaertner appeared. Worsnop(39), using heterogeneous radiation from a Coolidge tube, excited by unrectified current from a transformer, investigated the variation of the ratios S/P and S'/P' (defined as above) with increasing hardness of primary radiation, but found no evidence for the J-phenomenon. Gaertner(40), using a hot-cathode tube, excited by valve-rectified alternating current from a transformer, scattered the heterogeneous radiation from paraffin wax, and investigated the change in the ratio S/P with increasing thickness of filter. The results obtained afforded no evidence for the J-phenomenon.

There is thus a lack of agreement among the various observers. The J-phenomenon is as yet unexplained, and its apparently elusive nature hinders complete investigation. Also, in no case where the J-phenomenon is observed does Barkla admit that Compton's theory applies, though there is frequently a superficial resemblance. The Compton effect has, however, been supported by many experiments; by absorption measurements(30), spectroscopically by the use of both photographic(33, 34, 35, etc.) and ionisation(36) methods, and by observations on recoil electrons(37, 38, etc.); it thus rests on a very firm experimental foundation. In view of these facts it was thought desirable to repeat as far as possible the experiments of Barkla and others, with the view of obtaining further information on the J-phenomenon, but on account of the fact that in none of the papers published in support of the effect has there been given a full description of experimental arrangements, it has been a matter of some difficulty to ensure repetition of previous work. It was hoped, however, that by varying experimental conditions as much as possible, repetition of the main characters of Barkla's work could be assured.

§ 3. EXPERIMENTAL INVESTIGATION

The principle of the method employed was briefly as follows. A heterogeneous radiation was scattered from thin sheets of material, in a direction at right angles to the primary beam. The intensities of the scattered and primary radiation were compared by the balancing in turn of the ionisation currents produced by them against the current produced by a separate beam ("standard beam") from the same source. The amount of ionisation produced by this separate beam could be varied by limitation of the width of the beam by means of an adjustable slit. The penetrating powers of the radiations could be compared by measurement of the absorption in aluminium. The balance method used had the advantage of eliminating the effect of variations in intensity of the source, while variations in quality were shown up very clearly, owing to differences in the method of measurement of the "standard" and scattered beams. Throughout the experiments it was found that the limit of accuracy was fixed by the variations in the source, the apparatus being extraordinarily sensitive to variations in the tube potential caused by fluctuations in current through the primary of the induction coil that was used as a source of high voltage. This current, which could be kept constant to within 2 per cent., was obtained from the city mains at 230 volts d.c. The voltage of the supply was subject to sudden variations of the order of 5 per cent. Owing to shortage of apparatus, an induction coil with mercury interrupter was the only available source of high voltage. The use of lead cells to supply primary current effected a considerable improvement, but only a limited supply of these was available. Highvoltages were measured by means of a gap between 10 cm. spheres in air. The bulb, mounted in a support which enabled it to be turned at any angle, was enclosed in a large lead-covered box, lead screens serving to eliminate the effect of stray radiation.

The adjustable slit in front of the "standard" ionisation chamber was of iron 1·2 cm. thick, and was fitted with scale and vernier reading to 0·01 mm. Calibration of the slit by a timing method showed that, up to the maximum opening used, namely 1 cm., intensities were exactly proportional to slit-widths. Fine adjustment could be effected by means of a slow-motion screw. The "standard" ionisation chamber was of the two-electrode type, filled with air at atmospheric pressure. The beam of X-radiation passed through without impinging on the electrodes. The "primary" chamber was of exactly similar type during most of the experiments, but later was modified as described below. The area of aperture of this chamber was 2·5 cm.²

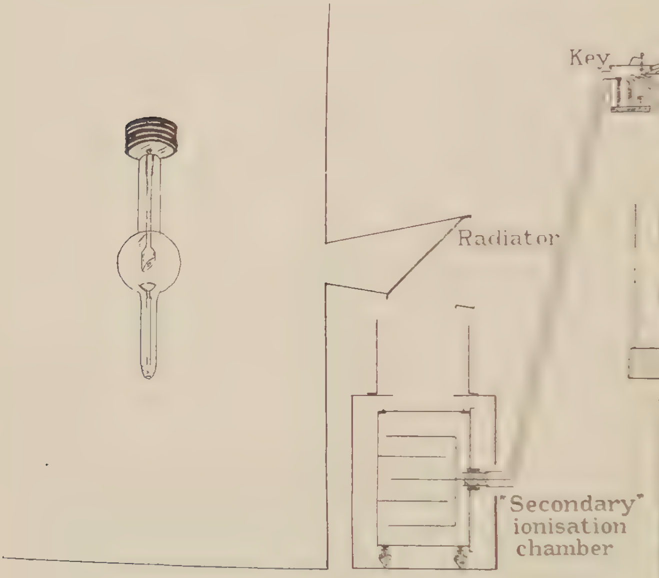

To electrometer
Key
To "standard" ionisation chamber
Radiator
"Primary" ionisation chamber
"Secondary" ionisation chamber

Fig. 3 (*a*). Electrical apparatus.

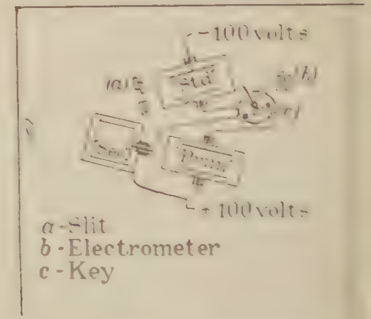


Fig. 3 (*b*). Diagram of connections.

The secondary chamber was of a different type in view of the fact that the radiation here was much less intense. The chamber was fitted with electrodes in the form of parallel plates placed normal to the direction of the radiation. The chamber was charged, and carried plates similar to those on the electrode and alternating with them, as shown in Fig. 3 (*a*).

These plates were made of aluminium foil, 0·005 cm. thick. The chamber was filled with air saturated with methyl iodide vapour. The apparatus used to ensure saturation of the air was designed also to prevent changes in the composition of the mixture with temperature and pressure, Fig. 3 (*c*). By raising and lowering of the movable reservoirs in turn, air was circulated from the fixed reservoirs back and forth through drying tubes of calcium chloride and phosphorus pentoxide, and through methyl iodide. By suitable adjustment of the stopcocks, the gas in the chamber could be shut off from contact with the liquid, in order to avoid disturbances due to changing vapour pressure, and was placed under slight pressure, 0·5 cm. of mercury, to eliminate the effect of slight leaks in the chamber, which was made airtight with soft wax. To indicate the position of balance a string electrometer was used, normally with potentials of 100 volts on the knife-edges, the sensitivity being about 500 divisions per volt. Since a null method was adopted, the most sensitive portion of the scale could be used. Balance could be obtained within less than 1 per cent. of the variation in width of the adjustable slit.

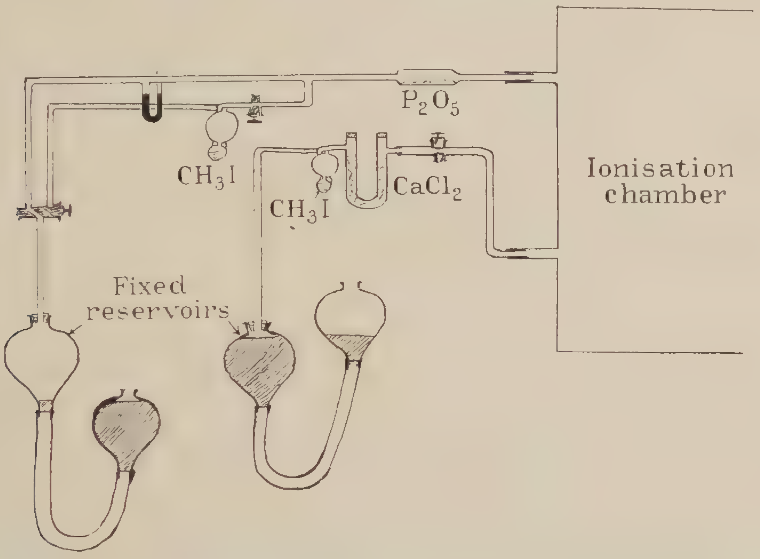


Fig. 3 (*c*). Apparatus for controlling composition of air-and-vapour mixture.

All ionisation chambers were enclosed in lead boxes fitted with levelling screws, to facilitate alignment with the beams. Wherever necessary, in order to avoid the effects of characteristic L-radiation from the lead, edges of slits, etc., were covered with a sufficient thickness, 0·7 mm., of aluminium.

The scattering material, in the form of thin sheets, was placed so as to bisect the angle between the incident primary and the scattered beam, as shown in Fig. 3 (*a*). This arrangement ensured approximately equal absorption of primary and scattered beams in the scattering material. Care was taken that no radiation, except that scattered from the desired material, should enter the ionisation chamber. Experiment showed that this stray radiation agreed in intensity and in quality with the radiation which would be scattered from the air in the vicinity of the radiator. The effect of polarisation of the primary beam was found by experiment to be small, and in any case was eliminated by arranging the direction of the cathode stream to make an angle of 45° with the direction of scattering.

§ 4. RESULTS

1. The primary radiation being kept constant, the same absorbing screen was inserted in the path of the primary and scattered beams in turn, and the intensities were determined. This procedure was repeated with different thicknesses of absorber. Figs. 4, 5 and 6 show the results of typical experiments. The scattered radiation appeared harder than the primary, but this fact may be explained by the method of measurement. It was considered that these differences in methods of measurement would be most likely to show up any difference between the radiations —differences which might be concealed by the balance method if the methods of measurement of the two beams were identical. Thus these experiments, while not calculated to measure differences in absorption coefficient, would certainly show discontinuities in absorption or sudden changes in the character of the radiation.

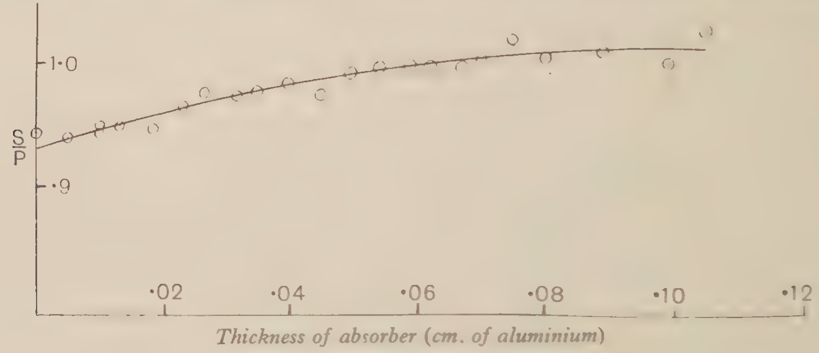


Fig. 4. Effect of filtering on S/P.

The experiments were repeated with primary radiations of various degrees of hardness, filtered by aluminium and by copper, unfiltered, from gas and Coolidge bulbs, with scatterers of aluminium, paper and paraffin wax; but in a series of more than forty sets of observations covering the range where the J-discontinuities have been observed, there did not once appear the slightest evidence for the J-phenomenon. On the contrary, curves similar to those in Figs. 4, 5 and 6 were obtained in every case. This result was independent of the frequency of the break and of the current through the tube.

Fig. 4 shows the effect of increasing filtering on the ratio of the ionisations produced by primary and scattered radiations, an unfiltered primary radiation of moderate hardness from a gas bulb being used. The mass absorption coefficient of the primary radiation varied with increasing thickness of filter from 2·4 to 1·8, thus including the position of the J_2 discontinuity ($\mu/\rho = 1{\cdot}9$). The ratio of ionisations increased uniformly throughout, the total variation being approximately

10 per cent. The points lie, within the experimental error of 2 per cent., on a smooth curve. Fig. 5 shows the results of a similar experiment, with a slightly harder primary radiation, the filtering being carried out to a greater extent. The same radiator, 0·35 cm. aluminium, was used, and the primary radiation was again unfiltered. The range of mass absorption coefficients was from 2·3 to 1·5, again including the position of the J_2 discontinuity. Again no trace of discontinuity appeared.

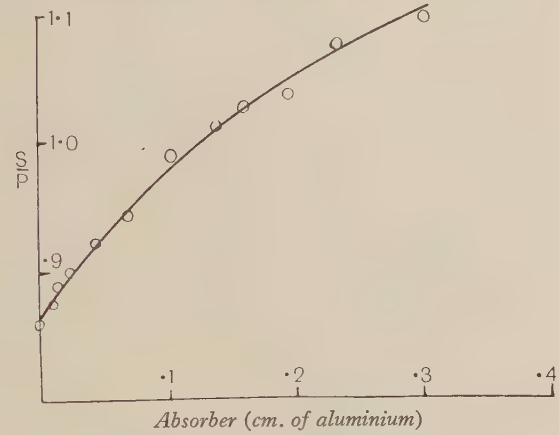


Fig. 5. Effect of filtering on S/P for harder radiation.

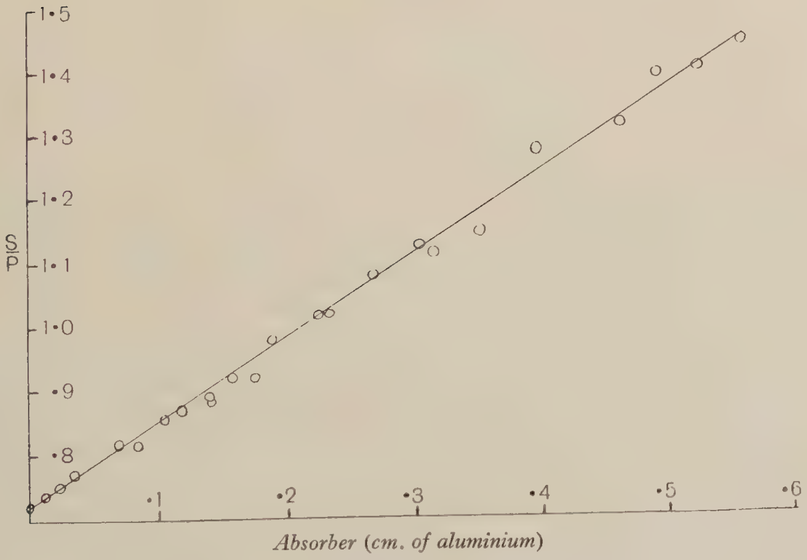


Fig. 6. Effect of filtering on S/P for still harder radiation.

Fig. 6 shows the effect of filtering on a harder radiation, and the absorption coefficients vary from 1·4 to 0·5, covering the position of the J_2 discontinuity

($\mu/\rho = 0{\cdot}7$). The points lie very nearly on a straight line. Variations up to 4 per cent. occur, but no trace of a discontinuity is observed. Owing to the smaller intensity of radiation obtainable, the experimental error was here greater, and was probably of the order of 3 or 4 per cent.

The upward trend of these curves, showing the scattered radiation to be relatively harder, may be explained as a result of the differences in absorption of the components of a heterogeneous radiation. The scattered radiation was absorbed in methyl iodide vapour, the direct in air. Two possible explanations are available, and probably both effects were operative: (1) the selective absorption in iodine, causing the harder components of the radiation to be more effective in producing ionisation, and (2) the greater absorption of all softer components of the beam in the aluminium plates and in the methyl iodide vapour, giving a filtering effect.

Both these effects could be avoided by making the methods of absorption of the primary and scattered beams similar. The "primary" ionisation chamber was modified accordingly so as to be exactly similar to the "secondary" chamber. The dimensions of the primary beam had now to be limited to those of pinholes in a lead screen, with a total area of 1·5 mm². Screens to eliminate lead L-radiation could not, of course, be employed in these circumstances. Experiments of a type similar to those already described now indicated that the secondary radiation was softer than the primary. Fig. 7 shows typical results.

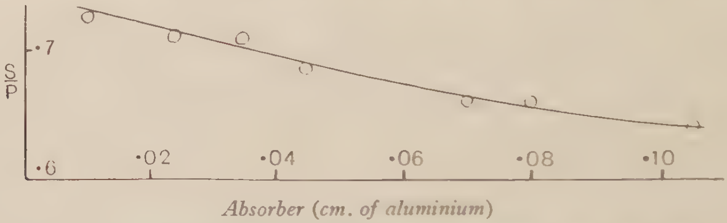


Fig. 7. Effect of absorption on S/P when the primary and secondary ionisation chambers are similar.

In a series of experiments with this arrangement of apparatus there likewise appeared no evidence of a J-discontinuity, but, on the contrary, results show a softening of the scattered radiation agreeing at least approximately with those predicted by Compton's theory. The theoretical amount of softening on Compton's theory could not be calculated exactly since the radiation employed was heterogeneous, of unknown energy-distribution, unknown amount of polarisation, etc.

2. The absorption coefficients of primary and scattered radiation, calculated on an approximately 50 per cent. absorption, were determined for different degrees of hardness of the primary radiation. The amount of softening produced was found to be greater than that observed by Dunbar, but this fact may be explained by the variation in scattering coefficient with wave-length, since the radiations employed were unfiltered, and thus heterogeneous. This view is supported by the fact that when filtered radiations were employed the change in absorption coefficient was found to be lessened.

These results are shown in Fig. 8, where the absorption coefficient of the scattered radiation is plotted against that of the primary. In the lower curve filtered radiations were employed.

3. The variation in the amount of softening produced by different thicknesses of scattering material was determined. It was found that the change in absorption coefficient of the scattered radiation from the thinnest sheets was within 6 per cent. of the change when thick sheets were used. This result was verified for aluminium, paraffin wax, and paper; Fig. 9 shows a typical case.

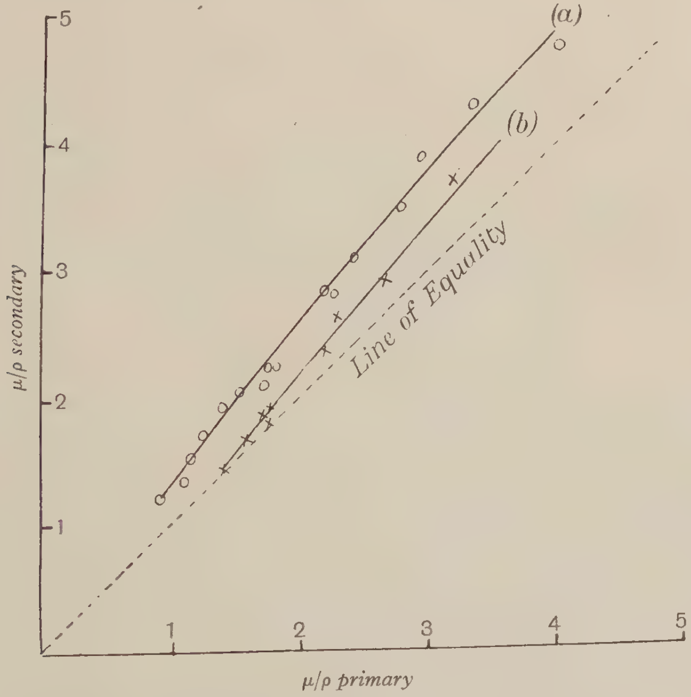


Fig. 8. **μ/ρ for primary and secondary radiation (aluminium).**
(*a*) Unfiltered radiation. (*b*) Filtered radiation.

Nine similar curves were obtained. Thus these experiments have failed to show the effect mentioned by Barkla in a letter to *Nature*(26), where he brings forward evidence that "modified scattered radiation is due to the superposition of unmodified scattered radiations." This effect is stated thus—"when the sheet of paper or paraffin wax was made gradually thinner, the difference between the primary and scattered radiations became smaller and ultimately almost vanished." "Plotting the change of absorbability against mass per unit area of the scattering sheet, we obtained curves exactly like the familiar ionisation-pressure curves showing saturation current."

The results obtained agree, however, with those obtained by Barkla, under apparently the same experimental conditions, when the radiation "settled down to a state where thick sheets, thin sheets, and even air itself produced a scattered radiation showing the full amount of modification."

4. The ratios of the absorption coefficients for copper and aluminium were determined for radiations of different degrees of hardness. The results obtained agree with those of Dunbar, and show similarly no evidence of a J-discontinuity.

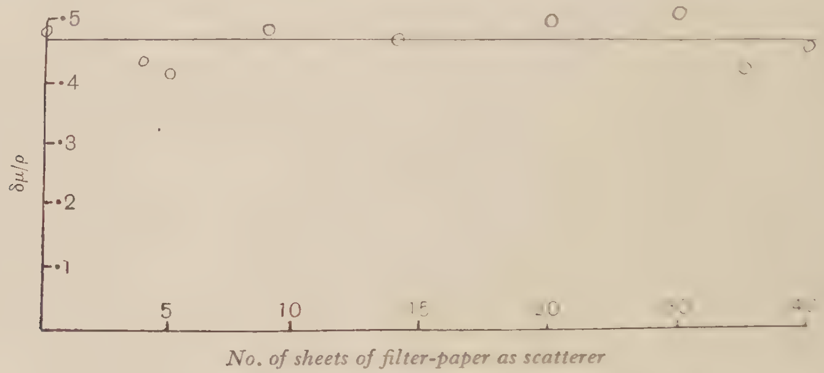


Fig. 9. Effect of thickness of paper on absorption coefficient.

§ 5. CONCLUSION

Thus in a wide range of experiments the results obtained show in no case the slightest evidence for the J-phenomenon, and confirm the work of Worsnop(39) and Gaertner(40). The only explanation of these results is that under the conditions of experiment the factors necessary for the appearance of the J-phenomenon have not been operative. It is apparent, therefore, that these factors are not ones which commonly occur, but are restricted to very special experimental conditions. This view is supported by the fact that, as the work of Barkla shows, even when the conditions necessary for the J-phenomenon have been realised, a very slight change is sufficient to cause the disappearance of the effect.

On account of the elusive nature of the phenomenon, and the difficulty experienced in realising the necessary conditions, it is desirable that full details of the experimental arrangements used by Barkla and his co-workers should be published. The lack of detail in the accounts published renders the exact repetition of the experiments impossible, and, until this repetition is ensured, it is impossible to investigate more fully the nature of the phenomenon, or even to substantiate its existence.

§ 6. ACKNOWLEDGMENTS

Finally, I wish to express my thanks to Prof. P. W. Burbidge, under whose direction this research was carried out, for his generous advice and assistance; to the Department of Scientific and Industrial Research, for a scholarship which enabled this work to be carried out; to Dr N. McDougall for his donation of most of the X-ray plant, and to the staff of the Physics Department for assistance generally.

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DISCUSSION

Mr I. Backhurst. Mr Alexander mentions (1) that the accuracy of his investigation was limited by the sensitivity of his experimental arrangement to variations of tube potential, and (2) that variations in the quality of the radiation were shown up very clearly by the balance method of measurement employed. These two effects are directly connected and as, in my opinion, due attention has not always been given to them by investigators of the J-phenomenon, I should like to make some observations as to how they come about. The distribution curve of spectral intensity for white radiation has a steep slope, as it rises from zero at the short wave-length limit and remains steep until it is comparatively near the wave-length of maximum intensity. The absorption coefficient in aluminium for any constituent wave-length λ is approximately proportional to λ^3, so that the greater the amount of aluminium placed in the path of the beam, the more is the transmitted radiation limited in the main to the short wave-lengths associated with the steep part of the spectral intensity curve. Clearly the intensity of these short wave-lengths is critically dependent on the position of the short wave-length limit and therefore on the potential applied to the X-ray tube.

White radiation, after transmission through a thickness x of aluminium, will have a percentage variation in intensity equal to K times the percentage variation of the voltage V applied to the X-ray tube, where K is a function of x and V which increases as x increases. If x is zero, K will commonly be about 2·5. If x is other than zero, K will usually have values up to about 10, and for heavily filtered beams may be 25 or more. When X-ray beams are compared in intensity by being balanced in turn against a beam of lower K value, effects (1) and (2), referred to above, are bound to appear, since two sensitive voltage indicators are being compared by means of a less sensitive one.

In conclusion I should like, if I may, to congratulate Mr Alexander on his success in obtaining results while hampered by difficulties such as arise from a fluctuating voltage supply: they are particularly inconvenient in a research of this kind.

ON THE DETERMINATION OF THE PARAMETERS IN AN EMPIRICAL FORMULA

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Communicated by J. H. Awbery, B.A., B.Sc., September 3, 1929.
Read by Mr Awbery and discussed December 13, 1929.

ABSTRACT. The writer reviews the method of least squares and gives reasons for recalling some cautions regarding its use that were mentioned by Stewart and Uhler several years ago. Examples are worked out to illustrate the discussion. The method recently mentioned by Awbery is compared with those of least squares and zero sum, from the practical point of view. Judging from the few examples given, the method of zero sum appears to be the quickest and its results to be well in accord with common-sense judgment. The amount of computation involved in the application of the various methods must be viewed in the light of the short cuts and elaborate calculating machines now available, since these may lighten the labour for one method more than for another. Attention is called to the fact that the method recently expounded by Awbery is the one given by Cauchy in 1847.

§ 1. INTRODUCTION

The subject of least squares and curve-fitting is so old that it is customary in this age to apologise for introducing the topic into the literature. Nevertheless the matter appears to be still of lively interest, for one may find a number of articles every year treating the subject. Papers of fundamental importance by Condon, Birge and Shea* have appeared within the past two years. These will be mentioned later. The recent publication of a paper by Awbery† suggests that there may be enough interest in the matter to justify the devotion of further space to it.

The primary purpose of this paper is to call to mind several facts regarding the method of least squares that seem often to be overlooked. The secondary purpose is to make two remarks concerning Mr Awbery's paper, and to make a few observations from a practical standpoint regarding the different methods, including Norman Campbell's zero sum method.

§ 2. COMMENTS ON THE THEORY OF LEAST SQUARES

The rule of least squares is based on two fundamental postulates: (1) the probability that an error will fall between two limits is a function of those limits; (2) in the case of several observations to determine a single quantity, the weighted average is the most probable value. These postulates are the mathematical equivalent of the Gaussian law of errors. Just as a system of geometry stands or falls with its postulates, so does the Gaussian law of errors and the method of least squares stand or fall with the two postulates stated above. Their validity will not be discussed here, except to remark that their experimental basis has been vigorously questioned at times. To admit them is to admit that the method of least squares gives the best solution to the problem at hand. (The question whether the method is ever worth while is another matter and is to be kept out of the argument at present.) How, then, can one countenance the two postulates and yet make remarks to the effect that in curve-fitting the method of least squares gives undue weight to certain points, or that the curve determined thereby is obviously out of place? Such remarks would then be an acknowledgment that the method has been misused. One might not wonder at an occasional abuse of the method in the adjustment of a set of observations, because the rules given in most textbooks treating of it do not warn the reader against likely pitfalls—in fact all the textbooks that I can find lead one directly into them. But one might well show astonishment at the confusion of ideas regarding the method that has been presented in some papers devoted to the subject of curve-fitting, and especially at errors in textbooks themselves. The prevalence of erroneous ideas concerning least squares was pointed out in two papers that appeared several years ago*†.

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* Condon, *University of California Publ. in Math.* 2, No. 4 (1927); Birge and Shea, *ibid.* No. 5.

† *Proc. Phys. Soc.* **41**, 384 (1929).

The commonest misuse of the method of least squares arises from throwing all the adjustment on to one set of observed quantities and none on the others, or from throwing it on to the wrong set. The simplest case to consider is that of observations subject to linear condition equations—the case of the straight line, as it would be called in curve-fitting. Suppose there are n sets of observed points $(x_1, y_1), (x_2, y_2), \ldots, (x_n, y_n)$. Let w_{xi} and w_{yi} be the weights of x_i and y_i; u_i and v_i their most probable values; and let

$$v_i = mu_i + b \qquad (i = 1, 2, \ldots, n) \qquad \ldots\ldots(1)$$

be the linear conditions to be satisfied. The method of least squares demands that

$$\Phi \equiv \sum_{i=1}^{n} w_{xi}(x_i - u_i)^2 + \sum_{i=1}^{n} w_{yi}(y_i - v_i)^2 \qquad \ldots\ldots(2)$$

be a minimum. Φ is a function of the $2n + 2$ quantities u_i, v_i $(i = 1, 2, \ldots, n)$, m, and b. For Φ to be a minimum,

$$\left.\begin{aligned} \sum_1^n w_{xi}(x_i - u_i)\,du_i + \sum_1^n w_{yi}(y_i - v_i)\,dv_i = 0, &\quad \text{1 equation} \\ m\,du_i - dv_i + u_i\,dm + db = 0, &\quad n \text{ equations} \end{aligned}\right\}.$$

Using Lagrangian multipliers $\lambda_1, \lambda_2, \ldots, \lambda_n$, we get the $2n + 2$ equations

$$\left.\begin{aligned} w_{xi}(x_i - u_i) + m\lambda_i = 0, &\quad n \text{ equations} \quad a \\ w_{yi}(y_i - v_i) - \lambda_i = 0, &\quad n \text{ equations} \quad b \\ \Sigma\, u_i\lambda_i = 0, &\quad \text{1 equation} \quad c \\ \Sigma\, \lambda_i = 0, &\quad \text{1 equation} \quad d \end{aligned}\right| \qquad \ldots\ldots(3).$$

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* H. S. Uhler, *Journ. Opt. Soc.* **7**, 1043 (1923).

† R. Meldrum Stewart, *Phil. Mag.* **40**, 217 (1920).

which with the n equations (1) are sufficient to determine the $3n+2$ unknowns, u_i, v_i, λ_i, m, and b.

The elimination of λ_i from (3 a) and (3 b) gives

$$\frac{y_i - v_i}{x_i - u_i} = -\frac{1}{m}\frac{w_{xi}}{w_{yi}} = \tan\delta_i \qquad \text{......(4)}.$$

The angle δ_i as thus defined is the angle that the line joining (x_i, y_i) with (u_i, v_i) makes with the x axis. If $w_{xi} = w_{yi}$, then $\tan\delta_i = -1/m$ and this line is normal to $v = mu + b$, the curve given by the least-squares adjustment. This line is parallel to the x axis if $w_{xi}/w_{yi} = 0$, and is parallel to the y axis if $w_{yi}/w_{xi} = 0$. So, the n observation equations $mx_i + b = y_i$ and weights w_i $(i = 1, 2, \ldots, n)$ being given, the usual rule given in textbooks that leads to the normal equations

$$m\Sigma w_i x_i^2 + b\Sigma w_i x_i = \Sigma w_i x_i y_i,$$
$$m\Sigma w_i x_i + b\Sigma w_i = \Sigma w_i y_i$$

can be used only when the observation on each x_i is considered infallible compared with that on y_i. When $w_{yi}/w_{xi} \neq 0$ it is not surprising that the m and b thus determined give a line that when plotted looks out of place. A graphical method is preferable to rules called least-square, which are merely devices for reducing the number of equations to that equal to the number of unknowns.

The above analysis was made on a linear relation in two variables, but similar remarks hold for any function. More general cases have been discussed by Stewart* and by Uhler* and need not be repeated. The conclusion is that the method of least squares does give a unique solution to the problem, and an alternative proposed method should not be compared with it unless it has been properly applied.

Before leaving this part of the discussion it is well to consider the following question. Given a set of observations (x_i, y_i) $(i = 1, 2, \ldots, n)$, is there any practical difference between throwing all the adjustment on to the x_i and all on to the y_i? To state the question another way—practically does it make any difference what is chosen to be "residual" in a particular problem? The answer is in general, yes, but a definite reply can be given only for a specific problem. Often the difference is so great that some other method may give intermediate results. A good problem† to consider is: "Six points, supposed to lie on the arc of a circle, have the following measured coordinates:

x	3·15	2·67	1·80	1·07	−0·20	−1·84
y	2·49	3·72	4·69	5·33	5·98	6·25

Find the most probable coordinates of the centre and the most probable radius." This problem as stated cannot be solved by the method of least squares, because there is no information about the relative weights of the observations. But for our immediate purpose that is of no moment; we should disregard such information anyway and work the problem by defining "residual" in two ways. Two different answers cannot both arise from the method of least squares; it happens here that neither of them does. Definition (*a*): the "residual" is the square of the tangential (if the point lies outside) or principal chordal (if inside) distance from the point to the circle. Definition (*b*): the "residual" is the radial distance from the point to the circle.

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* *Loc. cit.*

† Taken from Weld's *Theory of Errors and Least Squares*, p. 96 (Macmillan, 1916).

For comparison, the observation equations were written

$$x_i^2 + 2gx_i + y_i^2 + 2fy_i + c = 0 \qquad (i = 1, 2, \ldots, 6),$$

and solved by the method of zero sum*. The results are shown in Table 1.

Table 1. Results obtained with definitions (*a*) and (*b*) of "residual," and by the method of zero sum.

Method	*x*-coordinate of centre	*y*-coordinate of centre	Radius
(*a*)	−1·899	0·957	5·292
(*b*)	−2·050	0·775	5·496
Zero sum	−2·062	0·726	5·551

The circle determined by (*a*) is clearly out of place, though of course it fulfils the requirement laid down for it. Those determined by (*b*) and by the method of zero sum are scarcely resolvable, and each is a good guess, as can be seen by graphing them. This is to be expected because if one were to estimate the "best" circle, one would naturally be most concerned about radial distances. It should be kept in mind that none of the circles were determined by the method of least squares.

§ 3. REMARKS CONCERNING MR AWBERY'S PAPER

The method proposed by Awbery† shows promise of giving reliable results with less labour than that of least squares. It is easily extended to equations having any number of parameters, and, by the application of Taylor's series, to transcendental functions. It cannot be called new, however, because it appears to be a special case of a general method for any number of parameters, first given by Cauchy‡ in 1847. It was tested in this laboratory with the view of putting it into use. Awbery gives an example (I refer to his Example 1) comprising 26 observations on the linear variation with temperature *t* of the resistance *R* of a coil, where $R = at + b$. He solves for the parameters *a* and *b* by the method of least squares, throwing all the adjustment on to *R* (a procedure which I assume to be justifiable), and by Cauchy's method. The disparity between the times required for the two methods, as he gives them, seemed a little too great, so I handed the same problem to a computer in this laboratory. She was thoroughly familiar with both methods, but, to make the comparison as fair as possible, we reviewed the procedures carefully and she worked another problem both ways before starting on this one. The results are given in Table 2. Results by the method of zero sum, obtained several days later by the same person under similar conditions, are included.

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* Norman Campbell, *Phil. Mag.* **39**, 177 (1920); **47**, 816 (1924); *Physics*, ch. 17 (Cambridge Univ. Press, 1920); *Measurements and Calculation*, ch. 10 (Longmans, 1928). Campbell pointed out in the discussion of Awbery's paper that this method appears to have been given first by Tobias Mayer in 1748, but that it was forgotten until after Campbell's paper in 1920. However, except for the rule given by Campbell to govern the grouping of the observations in order to make the method unique, this method seems to be identical with the so-called "method of averages," which is explained in most books on empirical curves.

† *Loc. cit.*

‡ Augustin Cauchy, *Comptes Rendus*, **25**, 650 (1847). Reprinted in his *Œuvres Complètes*, Série 1, **10**, 412 (Gauthier-Villars, Paris, 1897).

Table 2. Comparison of computations by various methods.

Method ...	Least squares		Cauchy		Zero sum
	(i)*	(ii)†	(i)*	(ii)†	(ii)†
Time required	3 hr.	16 min.	1 hr.	21 min.	8 min.
Value of a	0·181620	0·181625	0·181601	0·181601	0·181594
Value of b	46·3459	46·3459	46·3463	46·3463	46·3465
Value of $\Sigma\,(R_{\text{obs}} - R_{\text{cal}})^2 \times 10^8/26$	12·1	10·5	12·7	12·7	11·9

* (i) denotes Awbery's results. † (ii) denotes author's results.

The discrepancy between Awbery's and our results in the "least squares" column must arise because we carried more figures; it is certain that the mean square residual obtained by the least-squares method must be lower than that got by any other. The times given under (ii) do not include the additional five minutes required in each method to calculate R and the residuals and to add their squares.

Two conclusions may be drawn from this tabulation. First, if Cauchy's method is shorter in general than that of least squares, then Awbery picked out the wrong kind of example to illustrate the fact. Our experience has been that Cauchy's method requires less time for most problems than does that of least squares, but unfortunately in those examples where we have found it to be much shorter, its results differed so much from those got by the least-squares method that we never felt enough confidence in it to adopt it. Second, the zero-sum method here appears most practicable because of its rapidity and because it meets the least-squares method on its home ground 10·5 to 11·9 (though I know Dr Campbell would object to judging it on the latter basis).

In the problem of the circle considered previously the results by the (b) and zero-sum methods were practically identical, but the latter required only a fraction as much time, because in method (b) the equations had first to be reduced to linear form. The few examples chosen for this paper do not afford sufficient grounds for recommending any method, but they do show that of zero sum to be worthy of consideration.

One further comment of a practical nature may be worth making. The mention of least squares usually summons up visions of vast amounts of computation, often considered not worth the trouble they involve. It may be that now, with the latest types of calculating machines, the shortening of the arithmetic involved in the method of least squares is out of proportion to the shortening effected in other methods. The least-squares adjustment of two variables, where the precision of one of them is so great that its error may be neglected in comparison with the error in the other, has been investigated by Condon*, who has explained how the computation in forming and solving the normal equations can be abridged considerably by making use of the orthogonal property of functions. Once tables for the application of Condon's scheme have been constructed for a particular type of problem, the saving in time is tremendous. The paper by Birge and Shea* illustrates what can be accomplished in this respect. By the use of Birge and Shea's tables for the type of problem to which they are applicable, the least-squares method becomes much shorter than any other yet proposed. Thus it is possible that our ideas regarding the amounts of labour necessary for the various methods may now need revision.

§4. ACKNOWLEDGMENT

In conclusion I wish to thank Miss Lola E. Shupe for making the calculations necessary in the preparation of this paper.

DISCUSSION

Mr J. H. Awbery (in presenting the paper). Dr Deming's results are summarised in his Table 2. I am surprised that the difference in times for the method of least squares should be so great, and suppose that it must be due to the use of Condon's method, with which I am not familiar. I repeated part of the calculation yesterday, and found that it took me 57 minutes to set up the normal equations, although without the checks that I would have applied if I had not been testing speed. Although I do not claim to be a computer, this still leaves a factor of something like 6; I did not know I was so bad. I am also surprised at the speed with which the calculated *R* can be obtained and the residuals formed, squared and added. Apart from the calculation, it is necessary to write down well over two hundred figures, and this would take me an appreciable portion of the five minutes allowed. However, on the times given, the conclusion seems to be that in my hands the method of Cauchy is quickest, and in Dr Deming's hands the method of least squares wins. I have no experience of the zero-sum method.

Dr Deming points out that the method brought forward by me† was first suggested by Cauchy. I am interested to find that this is so and am grateful to Dr Deming for pointing it out.

J. A. Hall. Having a certain interest in the resistance measurements which Mr Awbery used for his calculations, I have had the curiosity to repeat some of the work with the view of discovering the cause of the discrepancies between his and Dr Deming's values for the least-squares equation. My calculation, retaining all the figures that came, gave a value of 0·181621 for *a*. I have not had time, however, to check this calculation. I also investigated the difference between the values of 12·1 and 10·5 for the sum of the squares of the residuals. The value of 12·1 appears to have been taken from the values of the residuals in Mr Awbery's paper, and these residuals were only given to one significant figure. The extension to three figures in Σr^2 cannot, therefore, be justified. However, to clear up the difference, I re-calculated the residuals to three significant figures (though the additional figures have no experimental significance), using both Mr Awbery's and Dr Deming's least-squares equations. I obtained a value of 11·9 in the case of Mr Awbery's equation, and 11·8 in that of Dr Deming's equation. These calculations I have had an opportunity of checking, and I have not been able to find any error.

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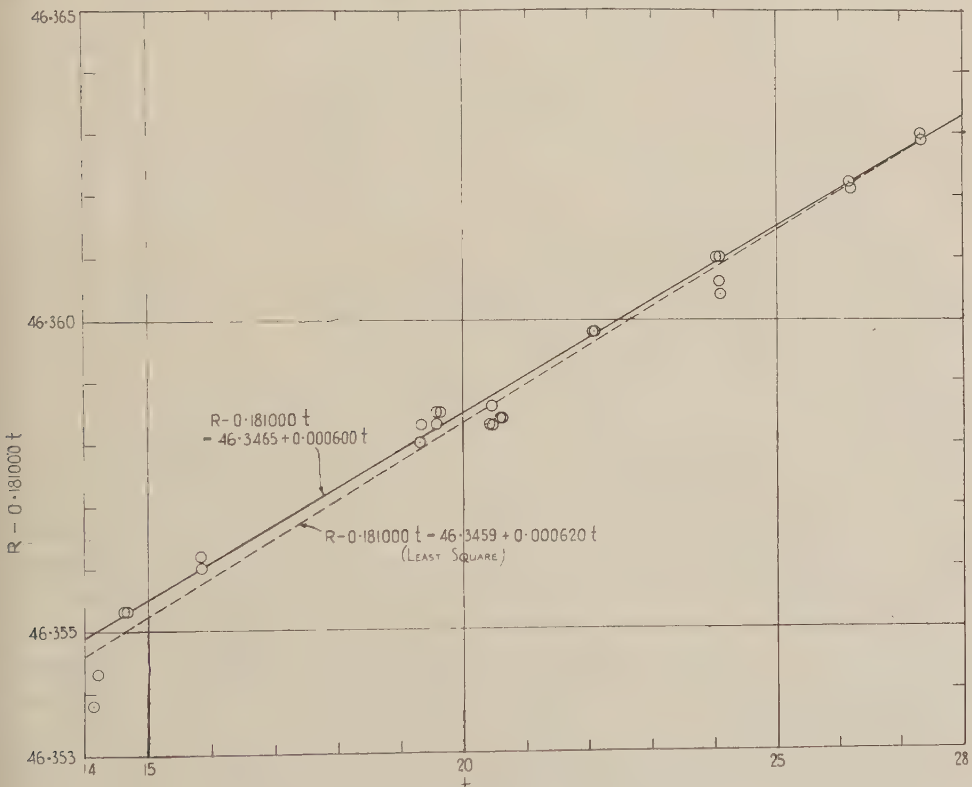
* *Loc. cit.*

† *Proc. Phys. Soc.* **41**, 384 (1929).

Mr A. F. Dufton. On p. 100 Dr Deming states that the circle determined by (*a*) is clearly out of place. This is by no means obvious to me, for the mean deviation of the points is only 0·019 compared with 0·040 for the circle found by the method of zero sum. If the points are plotted on one-inch coordinate paper, the position of the centre can be estimated with the aid of ruler and compasses. I find it to be (− 1·95, 0·90). From the circle with this centre and of radius 5·36 the mean deviation of the points is 0·020. On p. 101 Dr Deming compares mean-square residuals. The mean of the actual deviations would appear to afford a useful comparison. The mean deviation of *R*, both from the "least square" line and from the "Cauchy"



Graph illustrating Mr Dufton's remarks.

line, is $3{\cdot}0 \times 10^{-4}$. Computing $(R - 0{\cdot}181000t)$ and expressing it as a linear function of t by the graphical method which I recently described*, I find $R = 46{\cdot}3465 + 0{\cdot}181600t$, with a mean deviation of $2{\cdot}4 \times 10^{-4}$ (see the accompanying graph).

Mr T. Smith. Dr Deming's paper contains a number of assertions which appear to need the addition of important restrictions not admitted by him. In the second section it is claimed that two postulates of a very broad character lead inevitably to the method of least squares. It would be interesting to see how the latter is deduced from the former without the introduction of any additional condition, as, for instance, a limitation of "function." My expectation is that the task is impossible.

In his criticism of Mr Awbery's method Dr Deming gives times of operation which are remarkably short. I doubt if the quickest computer I have known, using a machine of one of the types available at the National Physical Laboratory, could approach them. One inevitably wonders whether accuracy is not sometimes sacrificed in such rapid working. That errors have been made in the example of the circular arc with radial residuals (the only illustration I have examined) can be seen by plotting the points on squared paper and drawing the circle. With the given centre the radius of the circle should be about 5·509 instead of 5·496. [Illustrative graphs were shown at the meeting.]

We should, I think, beware of adopting a single method of dealing with a variety of numerical problems without considering whether it is the most suitable one to employ. The method of least squares is often used when others should be preferred. Discrimination must rest on an experimental basis. Without implying that there are no fields in which the method of least squares and the Gaussian doctrine of errors are applicable, we may stress the fact that deductions which according to this theory should have been on a very secure basis (particularly the probable error of a mean) have at times turned out to be so far wrong that the theory cannot have been properly applicable to the observations. The application of a theory which assumes the absence of systematic errors is only too likely to mislead when we know nothing about these errors.

When the physicist fits a formula to a series of observations his aim is frequently to find a simple expression by which the latter can be represented with sufficient accuracy. He may perhaps know that the theoretical relation is of a different form. The formula enables him, on a basis of sampling, to obtain with little trouble numerical values that are accurate enough for the use he will make of them. In the absence of a suitable formula or graph he would have to tabulate observations at close intervals and use them in the same way as mathematical tables are used. The error should nowhere exceed a certain value, just as the error in tables should nowhere exceed one-half of the last figure recorded. A mathematical table with a few errors in the penultimate place would unquestionably be bad compared with one in which numerous errors not exceeding one unit in the last place were present, although by the criterion of least squares the former might appear much the better of the two. On such grounds we can see that a method of equalising the departures of the curve from the observations will often be preferable to fitting by the method of least squares. The procedure will then be to fit two parallel curves, lying as close together as possible, between which all the observations lie, and to adopt as the solution the curve lying evenly between them. No questions of weight or probability (which are usually guesswork) are involved. Applying this method to the problem of the arc, I find the centre at about $x = -1{\cdot}8597$, $y = 0{\cdot}9301$, with radius 5·2835. The radial departures, together with those obtained from Dr Deming's solution, and the concentric solution with increased radius already mentioned, are given in the following table:

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* *Nature*, **121**, 866 (1928).

Point no.	Equal departures	Deming	Modified Deming
1	− 0·0365	− 0·0205	− 0·0335
2	+ 0·0365	+ 0·0674	+ 0·0544
3	− 0·0365	− 0·0051	− 0·0181
4	+ 0·0026	+ 0·0251	+ 0·0121
5	+ 0·0322	+ 0·0280	+ 0·0150
6	+ 0·0365	− 0·0170	− 0·0300

The advantages of the equal departure method would be more apparent were a larger number of observations involved. In this example a comparatively small increase in the magnitude of the departures at points 1, 2 and 3 permits those at points 5 and 6 to be much reduced. This suggests that the Deming solution for least squares is incorrect in the position of the centre as well as in the radius.

Dr N. R. CAMPBELL (communicated). Of course I agree whole-heartedly with most of what Dr Deming says, but—(1) The postulate of least squares is not merely that the error is "a function of those limits"; it is that the error is a *continuous* function of them. This is a vital distinction, which all the orthodox treatises ignore—vital because the first postulate is probably true, the second most certainly false. (2) It is quite true that zero sum has little or no advantage over least squares to the skilled computer armed with all the weapons of her trade. But in such conditions the two methods are not alternative. If we are going to squeeze the last ounce out of our observations then there is no single "right method"; we must sit down first and inquire what is the best method for our particular problem; and if one method can be shown to be superior to another, then it would be criminal to reject it on the score of extra labour. The people who ought to use zero sum regularly are those who now never think of adopting any but the graphical method. If they would only realise that it is actually quicker to do the computation than to draw the straight line, everybody, except the manufacturers of squared paper, would benefit. (3) I wish that my name were not so persistently attached to zero sum; the impression arises thereby that it is merely one out of many fads. In my original paper I put it forward as the method that must inevitably suggest itself to anyone who had not lost his common sense by worship at the Gaussian shrine.

Mr J. H. Awbery (in reply to the discussion). I was interested in Mr Smith's remarks, and glad to hear that he too would find difficulty in working at the speed of a professional computer.

He objects to Dr Deming's statement of the postulates from which the Gaussian law of errors may be derived, and considers that it would be necessary to state definitely the function of the limits concerned in postulate (1). Actually postulate (2) does much to restrict the possible forms of function in (1), although to obtain the normal law it is necessary to assume in addition that the function is an even one (so that negative and positive errors of the same magnitude are equally likely). The question is discussed in detail in Keynes' *Probability*, pp. 195 *et seq.* and in Jordan's *Statistique Mathématique*.

Mr Hall's contribution to the discussion concerns me particularly, in connection with the value of the mean-square residual. The question is purely one of arithmetic, and it appears that Dr Deming and his computer have made some slight slip. I had not previously checked the facts as given by Dr Deming, but I now find that Mr Hall is right. The mean-square residual by least squares is about 11·8, and not 10·5, if sufficient figures are carried.

Mr Dufton puts forward the claims of his method recently expounded in *Nature*. It may be of interest to point out that his method, applied to a single quantity, assumes that the median (i.e. the value which occupies the middle place when the observations are ranged in order of magnitude) is the most probable value. The corresponding law of error is $\kappa e^{-h x}$. The claims of the median were recently put very powerfully by Edgeworth in *Phil. Mag.* **46**.

Author's reply (communicated). Condon's method was not used. Probably the disparity in time arises in the amount of pencil work. Miss Shupe read the figures directly from the June 15 *Proceedings*, and the only thing she put on paper was the normal equations and their solution. Sums of products can be formed in the machine without recording each one. Ordinarily we use the scheme for checking described in Leland's *Practical Least Squares*, but for this work we used a less trustworthy check in the formation of the normal equations, to shorten the time (*vide infra*). With the method that we used for calculating the mean-square residuals the only numbers recorded are the residuals, and if only one significant figure is used it is necessary to write only 26 figures, not 200. The 26 residuals can easily be computed in five minutes.

Mr Hall is perfectly correct; the value 12·1 came directly from Mr Awbery's residuals, which were given to one figure only. Our 10·5 was derived in the same way, because we wished to follow Mr Awbery, though it is probably more common to calculate one figure beyond the observations. Also, Mr Hall's value of a is correct; the discrepancy that he mentions came from our careless throwing away of a few decimals almost at the very end of the solution of our normal equations. We find, however, that this does not affect our residuals, which were calculated to one figure, so our value 10·5 stands.

In regard to the time of computation, I should like to add that on January 17, 1930, Miss Shupe repeated the task of finding *a* and *b* by the method of least squares, to check Mr Hall's value, and her time was 14 minutes—2 minutes less than it was previously. For a problem like this, the time would be nearly doubled if the sum check mentioned above were included, but in a problem involving more parameters the time would not be increased in so great a ratio.

I should like to add to Mr Smith's remarks by saying that the method of least squares, unlike most other methods, professes to take account of the weight of each observation. It not only finds "most probable" values for the parameters, but it also adjusts the observations. Now unless weights can be assigned to the observations with some confidence, the solution by least squares cannot even be commenced, and some other method must be used.

I feel under great obligations to Mr Awbery for communicating my paper to the Society, and to the Editor for the opportunity of making these final remarks.

DISCUSSION ON PRACTICAL EXAMINATIONS IN PHYSICS

In which the Science Masters' Association participated.

A NEW TYPE OF PRACTICAL EXAMINATION, DESIGNED TO BE A FAIR COMPETITION

By L. F. RICHARDSON, D.Sc., F.R.S. and R. S. MAXWELL, M.A., B.Sc.

Paper circulated before the meeting.

ABSTRACT. This new type of examination in practical physics consists of a number of short experiments. Each candidate attempts every experiment. There are two advantages:—(i) It is a fair competition between candidates. (ii) For every candidate the tests can be scattered widely over the syllabus. The correlation coefficients between success in practical physics and successes in other tests have been found by means of this examination to be: for theoretical physics $0{\cdot}4_6 \pm 0{\cdot}0_9$, for pure mathematics $0{\cdot}1_9 \pm 0{\cdot}1_6$, for the General B.Sc. $0{\cdot}2_7 \pm 0{\cdot}1_5$, for practical teaching $0{\cdot}0$, for Spearman's g $0{\cdot}3_5$. The correlation of g with theoretical physics is much higher. The numbers following the ± sign are standard errors.

§ 1. GENERAL DESCRIPTION AND ADVANTAGES

THE practical examinations in physics set by the University of London to external candidates consist of two experiments, each of 1½ hours for the Intermediate Science, of 3 hours for the General B.Sc., and of 6 hours for the Special B.Sc. The experiments are not the same for all candidates; out of about 16 kinds of experiments 2 are allotted more or less at random. This kind of examination is, as far as we know, typical of present practice in physics.

One of us (L.F.R.) having tried to mark college examinations of this type during 5 years, and having found it very difficult to put the men in order of merit, suggested in 1925 that the experiments should be briefer so that they could be made the same for all candidates, and more numerous so that they could be spread over the syllabus. Something of this sort has long been done elsewhere when candidates in medical or psychological examinations each in turn inspect the same museum specimens. We have shared about equally in the conversion of this idea into a regular practice for physics and in the discussion of the results of eight such examinations held at Westminster Training College, London, S.W. 1.

It will be convenient to have a name for the new type of examination; and we suggest pantopiric* from Greek *πειράω* to attempt and *πᾶν* all, because each student tries all the experiments. Locally we have called it "progressive games."

As an example let us suppose that 20 men are to be examined and that the pantopiric examination is to last 3 hours. Then 20 experiments (plus two spares lest any apparatus become broken) must be devised, each lasting 1/20 of 3 hours, that is 9 minutes. The apparatus is arranged on the benches in the laboratory during the two previous days, and beside each set is fixed a card bearing conspicuously the number of the experiment and, in smaller writing, brief instructions to the candidate, amounting usually to between 5 and 40 words. Five minutes before the examination begins, the candidates are arranged in alphabetical sequence in a passage outside, and each is given a booklet of blank paper and is told the number of the experiment at which he is to begin. The doors are then opened. After nine minutes a bell is rung and an invigilator calls loudly "Move on to next larger number!" There is usually rather a muddle at first, as some candidates are so deep in their work that they do not hear the order; but after the first hour all goes smoothly. Every nine minutes the bell is rung, orders are scarcely necessary, and there is a general commotion lasting only about 30 seconds. The invigilators keep a watchful eye on apparatus to see that it has not moved out of adjustment, and in each nine minutes can find time if necessary to check one or two settings, say one on a spectrometer, one on an optical bench. Perhaps an apparatus is broken and its number has to be transferred to one of the spares. Apart from such an accident, at the end of three hours every candidate has attempted every experiment. The candidates usually look rather tired at the end.

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* We are indebted for this name to our classical colleague Mr F. C. G. Langford.

Actually the examinations have been held, inconveniently, in two groups of rooms connected by a long passage. One of us invigilated in each portion, and at the sound of the bell two candidates hastened in opposite ways along the passage. Special labels were put to direct them.

Because all candidates have done the same experiments we have found it possible to put them in order of merit with a confidence unknown in the ordinary type of examination. Marking has occupied two examiners for perhaps 6 to 10 hours because the examiners have had to perform the experiments to find out (i) what is correct, (ii) what marks to give to various deviations from the correct value. We have usually assigned a maximum of 5, 7 or 10 marks to each question.

At this stage the reader may well be in doubt, as we were when we began, as to whether an experiment lasting for so short a time as nine minutes can have any interest or value as a test.

Psychologists have for a decade been using such short practical tests*. We have found it possible to arrange brief tests of this type so that they could be either easy for a student who has only taken Matriculation physics or difficult for a student who six months later took honours in the General B.Sc. Further, there is no indication that we have reached the limits of ease or of difficulty. The task has been to select tests:

(i) Of a difficulty such that one small fraction of the class will obtain full marks and another small fraction zero marks for each test, and

(ii) So as to cover the syllabus, or such portions of it as the students had studied.

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* See, for instance, *Performance Tests of Intelligence*, by F. Gaw. (Industrial Fatigue Research Board, Report No. 31, 1925.)

As the devising of these tests seems to be the chief original part of this work, we set out fully in § 6 the instructions that were placed before the candidates. To describe the apparatus would take too long; we are compelled to leave this mostly to the imagination of the reader aided by brief notes in square brackets. These notes were not shown to candidates.

To show how these tests discriminate between different students, let us consider a few tests in detail.

Example 1.

Class. Second year after Intermediate, 22 men. Dec. 1925.

Time allowed for experiment, 8 minutes.

Instructions. "Adjust the spectrometer so as to show the two yellow lines of sodium separate, and show them to the examiner."

Apparatus. Hilger spectrometer, 60° prism about 4 cm. in edge, of square face, bunsen sodium flame, dark room.

Inspection. An invigilator inspected each student's work and afterwards put telescope and collimator out of focus and disarranged the prism. Such need for inspection and disarrangement has been avoided in 9 tests out of 10.

Failures and successes. Some candidates, flustered or unskilled, did not succeed in finding any spectral image. Others, more practised, concerned themselves with the minimum deviation. This was unnecessary, as they were not asked to measure angles. A few, having found an image, simply focussed it carefully by Schuster's method, when the lines were seen double.

Example 2.

Class. First year after Intermediate, 12 candidates. March 1928.

Time allowed for experiment, 15 minutes.

Instructions. "Find the cooling constant for the given ball, i.e.

$$\frac{\text{rate of cooling in degrees C. per minute}}{\text{excess temperature of ball over surrounding air in degrees C.}}\text{"}$$

Apparatus. Copper ball, about 6 cm. in diameter, suspended by a wire. Bunsen for heating it. Two thermometers, the bulb of one in a deep hole in the ball, the other in the air protected from the ball by a screen. A stop-clock.

Failures and successes. One candidate overheated and destroyed the thermometer. Several others observed temperatures every minute, worked differences and then averaged them so as unconsciously to eliminate all except the first and last readings. Several others used the well-known better arithmetic in which all readings have weight in the result.

Example 3.

Class. Second year after Intermediate, 13 candidates. December, 1927.

Time allowed for experiment, 13 minutes.

Instructions. "Measure the resistance and self-inductance of the given solenoid using the Campbell variable standard."

Apparatus. Heaviside equal-ratio bridge all ready connected with buzzer and telephone.

Failures and successes. This test is obviously too difficult for those who have not passed the Intermediate B.Sc. But it can easily be done in half the time allowed by anyone familiar with such measurements. Those who have only a slight acquaintance do not realise that there are two adjustments which must be made alternately many times in succession, until both are made right together. Also the doubling of the inductance is often forgotten. So far it is a test of knowledge rather than of dexterity; but an accurate setting depends on a sensitive ear.

Example 4.

Class. Intermediate, 21 candidates. December 1926.

Time allowed for experiment, 8 minutes.

Instructions. "Set the upper surface of the glass plate horizontal. Show your setting to the examiner."

Apparatus. Plate glass 25 cm. square resting on three levelling screws. Clinometer, Field, Mark III, purposely about 10′ out of adjustment.

Failures and successes. A few students were entirely bewildered. Common mistakes were (i) to assume the clinometer to be perfect, and (ii) to set the spirit level in a random azimuth.

The experiment could be performed in half the time allowed if the clinometer were first set parallel to the line joining two of the three feet and then turned in azimuth successively through 180°, 180°, 90°, 180°.

We have on one or two occasions given a class the choice between the pantopiric and the ordinary examination, and they have chosen the pantopiric, saying that it was fairer.

§ 2. THE METHOD OF MARKING

To describe in detail the method of marking each experiment, as is always done for standardised psychological tests*, would occupy too much space. But a brief survey of the actual system can be made by counting the frequency with which the several marks 0, 1, 2...10 have been given for work for which the greatest possible mark is 10. Table I shows this.

Table 1.

Mark	10	9	8	7	6	5	4	3	2	1	0
Frequency	136	108	110	93	103	98	115	114	100	78	383

At any one examination there are two purposes for which marks may be assigned, namely:

(i) To show whether the candidates have or have not attained a prearranged standard, say that set by the University of London. For this purpose it might be quite correct to give all the candidates no marks or all full marks.

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* Terman, *The Measurement of Intelligence.*

(ii) To put the candidates in a succession of merit; and further, if the candidates are numerous, to choose a scale which distributes the marks according to some known law, say the Gaussian normal frequency distribution, as was done by K. Pearson in an investigation of the inheritance of ability*.

These two purposes (i) and (ii) tend to coalesce when the whole academic life of the country is considered over a decade. For the Universities do in fact choose standards which sort the candidates into a sequence of merit: 1st class honours, 2nd class honours, pass and fail.

§ 3. DIFFICULTIES AND OBJECTIONS

Large numbers of candidates. Our largest group numbered 36, and we feel that this number is almost the practical limit for a single circulation during 3 hours; for with it the time allowable for each experiment is only 5 minutes, which is inconveniently short.

On two occasions we duplicated each experiment and the candidates circulated in two self-contained "vortices." This plan could obviously be developed for dealing with large numbers of candidates when apparatus is abundant.

Heat experiments are, many of them, too slow to fit into 10 minutes. Nevertheless, we have found a few, to be seen in § 6. This is, however, one of the serious weaknesses of the pantopiric examination.

Correlation with ordinary examinations. To what extent is success in 10-minute experiments correlated with success in thinking out a chain of complicated experiments lasting all day, such as those set in the Special Physics examination of the University of London? We do not know. The reliable way of finding out would be to test, say, 30 students both ways, and to work out the correlation coefficient. It might be supposed that the written examination would provide sufficient opportunity for testing powers of persistent logical thought; but in view of Professor Spearman's† destructive criticism of arbitrarily assigned "faculties" we have not much confidence in this supposition.

Perseverators‡ may be at a disadvantage. This objection, made by Mr G. E. Whiting, deserves consideration.

§ 4. RELIABILITY

An examination would not be considered reliable if the same group of candidates, on being examined twice, came out in quite different sequences. The reliability of the pantopiric examination is measured by a "reliability coefficient" defined, according to the practice of psychologists§, to be the correlation coefficient between the marks obtained by the same candidates in two examinations, both of which are pantopiric, although differing in the detail of the questions set.

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* K. Pearson. *J. Anthrop. Inst. G.B. and I.* **33**, 179 (1903).

† C. Spearman, *The Abilities of Man* (Macmillan and Co. 1927), chap. 3.

‡ *Ibid.* chap. 17.

§ G. M. Whipple, *Manual of Mental and Physical Tests*, 1, 51–53 (1914). W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, p. 132 (Cambridge University Press, 1925).

As the candidates' state of training might vary during an interval between two examinations, the latter are taken to be the tasks having odd and even numbers, all performed during the same 3 hours. Computing all coefficients by product moments, we obtain Table 2.

Table 2.

Date	Examination	No. in class n	(Half-exam.) r_1
1925 (Dec.)	Final II	22	0·74
1925 (Dec.)	Intermediate	16	0·47
1926 (Mar.)	Final I	12	0·34
1926 (Dec.)	Intermediate	21	0·72
1927 (Mar.)	Final I	14	0·43
1927 (Dec.)	Final II	13	0·17
1927 (Oct.)	Intermediate	36	0·45
1928 (Mar.)	Final I	12	0·59

The best way of forming the weighted mean of such correlations appears to be R. A. Fisher's, according to whom the mean* is

$$\tanh\left\{\frac{\Sigma\,(n-3)\left\{\tanh^{-1} r_1 - \dfrac{r_1}{2\,(n-1)}\right\}}{\Sigma\,(n-3)}\right\} = R_1 \text{ say.}$$

Here R_1 comes to 0·52.

But a whole examination is more reliable than either half of it, in fact, according to a formula due to William Brown†, the reliability coefficient R for one whole examination is

$$R = \frac{2R_1}{1+R_1} = \frac{1{\cdot}04}{1{\cdot}52} = 0{\cdot}68.$$

This is the reliability of a pantopiric examination lasting 3 hours. For pantopiric examinations lasting 6 and 12 hours the reliability coefficients may be computed by repeated application of Brown's formula, and come to 0·81, 0·89.

It is interesting to compare the above results with those obtained when the same students were re-examined pantopirically after an interval of about a year: see Table 3.

Table 3

Pair of examinations	No. of students	r
Intermediate, 1925 Dec. Final I, 1927 Mar.	4	0·97
Final I, 1927 Mar. Final II, 1927 Dec.	13	0·74
Intermediate, 1926 Dec. Final I, 1928 Mar.	5	0·39

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* R. A. Fisher, *Statistical Methods for Research Workers*, pp. 163–177 (Oliver and Boyd, 1928).

† Brown and Thomson, *loc. cit.*

Mean R, by Fisher's process aforesaid, is 0·73 and agrees well with 0·68 found above for simultaneous examinations of the same length, namely three hours.

What, we wonder, is the correlation between the marks in the first and second experiments of an examination in which different candidates have each two different tasks assigned to them, as in the ordinary University examinations? We have only one such for which the marks were kept separately, namely: Finals II, March 1925—10 candidates, $r_1 = 0{\cdot}47_4$, $2r_1/(1 + r_1) = 0{\cdot}64$.

This examination lasted 3 hours.

Reliability coefficients must be interpreted with caution, for they can be made large or small, for a given type of test, by choosing the group of examinees to have a large or a small range of the appropriate ability. To illustrate this, imagine a group composed of 50 navvies plus 50 physicists who had recently obtained first class honours. In successive pantopiric examinations the marks would tend to be nearly 0 per cent. for the navvies and nearly 100 per cent. for the physicists on each occasion. Consequently the reliability coefficient, computed via the product moment, would be close to unity.

In our groups, on the contrary, the individuals were much alike, in so far as all had matriculated and none had won scholarships to universities. In the final classes there is the further selection involved in previously passing the Intermediate Science. This uniformity must tend to make the reliability coefficients small.

§ 5. CORRELATIONS WITH OTHER TESTS

Before coming to the facts, it may be well to remark on meaning. If two examinations were found to have perfect correlation (coefficient $r = 1{\cdot}00$) then one or other of them is a waste of time and should be discontinued.

Correlation with theoretical physics as tested by college examinations

There has been nothing novel about these theoretical tests, in fact they have often consisted of old examination papers of the University of London. The correlation coefficients are set out in Table 4.

Table 4. Correlation between marks in college examinations on theoretical physics and marks in a pantopiric examination

Date	Class	n	r
1925 (Dec.)	Final II	22	0·42
1925 (Dec.)	Intermediate	16	0·41
1926 (Mar.)	Final I	12	0·34
1926 (Dec.)	Intermediate	21	0·53
1927 (Mar.)	Final I	14	0·25
1927 (Dec.)	Final II	13	0·09
1927 (Oct., Dec.)	Intermediate	35	0·69
1928 (Mar.)	Final I	12	0·28

Notes. Final I is the class consisting of those men who are in their first Final year.

Final II is the class consisting of those men who are in their second Final year.

n is the number of persons in the group who were present at the examinations in both theory and practice.

r is the correlation coefficient by the product moment.

The best value of the correlation coefficient is $R = 0{\cdot}46$ by Fisher's process described in § 4, and the standard error of $\tanh^{-1} R$ is according to Fisher

$$\{\Sigma\,(n-3)\}^{-\frac{1}{2}} = 0{\cdot}09.$$

Correlation with pure mathematics at the external examinations of the University of London

The numbers in the body of Table 5 give the numbers of students in the groups defined in the margins.

Table 5

	More successful in pantopiric examination	Less successful in pantopiric examination	Totals
Passed in pure mathematics	39	32	71
Failed in pure mathematics	14	19	33
Totals...	53	51	104

The tetrachoric correlation coefficient comes to 0·19 with a standard error of 0·16*.

Fourfold tables, like the above, were prepared separately for three Intermediate classes and three Final classes of students. The sequence of merit in the pantopiric test was divided into two portions equal or nearly so. The mathematical examinations followed the pantopiric examinations at intervals of 6, 7 or 8 months. But these separate tables showed nothing that is not better shown in the above table, which is their summary.

The smallness of the correlation between success in practical physics and success in pure mathematics tends to explain and justify the European custom of appointing in each university a pair of professors, one for experimental physics, the other for theoretical physics. For the obstacles encountered in research in theoretical physics are much more mathematical than the difficulties of the "theoretical physics" in the General B.Sc. which are largely a matter of memory and of description.

Correlation with success in the External General B.Sc. or Intermediate Science of the University of London

We divided each class into (i) successes including all those who passed the university examination at the first attempt in all their subjects with or without honours, and (ii) the rest. Each class was also divided into two equal or nearly equal parts according to the marks in a pantopiric examination held during the ten months preceding the university examination. Then the statistics of the separate classes were summed. The correlation no doubt depends on the subjects, other than physics, taken in the university examinations. For the Intermediate Science these were pure mathematics for all, together with any two chosen from applied mathematics, chemistry and geography. For the Final B.Sc. the subjects, other than physics, were pure mathematics for all, together with any one chosen from applied mathematics, chemistry and geography. In each compartment of the following table the upper number refers to the Final B.Sc., the lower to the Intermediate Science.

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* Obtained by the aid of K. Pearson's *Tables for Statisticians and Biometricians*, Tables 29, 23, 24. (Cambridge University Press, 1914.)

Table 6

	Pantopiric		Totals
	Better marks	Worse marks	
Successes in university examinations	14 18	14 8	54
The rest	8 13	6 23	50
Totals...	53	51	104

For the sum of Intermediate plus Final the tetrachoric correlation coefficient is 0·27 with a standard error of 0·15.

Here we must beware of casting undeserved aspersions. Success in an examination, in which written work predominates, has been found to have a small correlation with success in practical work; but there is yet the further question as to which kind of success is the more desirable. As a manufacturing nation our livelihood depends largely on practical skill*.

Correlation with success in practical teaching

Our students, being teachers in training, went into elementary schools for some weeks during their college course, and were there graded according to success as teachers by J. S. Ross, M.A., Lecturer in Education, to whom we are indebted for the marks. To find the correlation, we first prepared fourfold tables for four separate pantopiric examinations and then summed the four tables, obtaining Table 7.

Table 7

Practical teaching	Pantopiric physics		Totals
	High marks	Low marks	
High marks	19	18	37
Low marks	19	21	40
Totals...	38	39	77

Evidently the correlation is negligibly small. This might have been expected, for success in practical teaching depends vitally on many qualities which have little influence in practical physical work, namely on dress, voice, posture, pleasantness, strictness and so on.

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* See Presidential Address and proceedings of Section J, British Association, Glasgow, 1928.

It is regrettable that the dates at which the teaching marks were assigned varied from 0 to 2·3 years after the date of the pantopiric examination; but this is hardly likely to account for the negligibly small correlation.

Correlations with Spearman's general intellectual factor g

For a long explanation and defence of the importance of g we may refer to Spearman's book, *The Abilities of Man**, and especially to p. xvi, where we find

$$r_{ag} = (r_{ab} \cdot r_{ac}/r_{bc})^{\frac{1}{2}},$$

in which the r denote correlation coefficients between the measurements indicated by the suffixes; and a, b, c are almost any mental measurements. For instance, let a denote marks for pantopiric physics, b marks in theoretical physics, c marks in pure mathematics. Then it has already been shown on page 115 that $r_{ab} = 0{\cdot}46$ and that $r_{ac} = 0{\cdot}19$. So in order to compute r_{ag} we need also r_{bc}. It can be found from Table 8, which summarises Inters and Finals.

Table 8

	University pure mathematics	
College theoretical physics	Passed	Failed
Upper part	47	5
Lower part	25	26

By Pearson's tables (*loc. cit*) the tetrachoric r_{bc} comes to 0·70 with a standard error of 0·10.

Then by Spearman's formula, used in three ways, it is found that the correlations with g are:

g and theoretical physics	1·30
g and pure mathematics	0·54
g and pantopiric physics	0·35

The large correlation of g with theoretical physics would be gratifying to physicists if it were not impossibly large. The error can be explained by the large fractional uncertainty of $r_{ac} = 0{\cdot}19$ as shown by its standard error of 0·16.

§ 6. LIST OF EXPERIMENTS

Notes: (i) Explanations in square brackets were not shown to candidates.
(ii) The December examinations for Intermediate Science are on one term's work; hence the narrow syllabus from which electricity and magnetism are excluded.

Intermediate, 1925 *December. 22-minute experiments. Two "vortices"*

1. Find the periodic time T of the given pendulum under two circumstances: T_0 for small amplitudes; T_1 for a mean amplitude of one radian. Work out $(T_1 - T_0)/T_0$.

2. (*a*) Find the focal length of the given lens. (*b*) Measure the magnifying power of the lens directly, and compare your result with the theoretical formula. N.B. Remember that the virtual image produced by the lens must be at a distance of 25 cm. from the lens, for your comparison to have any meaning.

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* Macmillan and Co. 1927.

3. Identify the ten specimens *A*, *B*, *C*, ... *J*, and state for what purpose they are used. [Specimens: *A*, travelling microscope; *B*, cork borer; *C*, silk band of Calendar's apparatus; *D*, rider of Atwood's machine; *E*, trolley of Fletcher's apparatus; *F*, Dewar flask; G, lump of solder; *H*, stop-watch; *I*, glass prism; *J*, copper calorimeter.]

4. Round off the sharp edges on the ends of the piece of glass tubing. Bend the tube at right angles. Write your name on a label. Stick the label on the tube. Hand the tube to the examiner.

5. Determine the water equivalent of the copper bottle, without weighing it. What improvements would you make in the apparatus if you had more time?

Provided also: one thermometer, 100 cm.^{3} glass jar, access to water tap, bunsen, tripod, stopper, 3 large corks, tape.

6. Find the weight of a metre scale.

Provided: metre scale, support, thread, 200 gm. weight.

7. Find the radius of curvature of the given concave mirror. Show the correct setting of the apparatus to the examiner before you move it out of adjustment.

8. Find the freezing temperature of the given substance.

(Spare.) Find the mass and the diameter of the given sphere, and thence calculate its density.

Provided: balance and larger weights, screw micrometer, log tables.

Intermediate, 1926 *December*. 8-*minute experiments*

1. Find the refractive index of the given liquid for NaD light.

[Total internal reflection by air-film between parallel plates.]

2. Using the given prism and spectrometer, find the spectrum produced by the light from the electric light provided. Show the spectrum to the examiner. N.B. Do not alter the focussing arrangements of the spectrometer.

3. Measure the specific gravity of the given liquid, using the common hydrometer.

4. Find the focal length of the given lens.

5. Measure the temperature recorded by the wet bulb thermometer, and take the temperature of the surrounding air. How could these observations be used to determine the hygrometric state of the atmosphere, and what further data would be necessary before you could calculate this?

6. Bore a hole in a cork, and fit through it the large glass rod provided. Show the completed experiment to the examiner.

7. Adjust the Fletcher's trolley so that it runs with a constant velocity. Write your name on the trace you have obtained, and leave it on the bench, on the space provided.

8. Make a rectangular bend in the piece of glass tubing provided. Fix a label with your name written on it on to the tube, and leave it in the place indicated.

9. In measuring an angle of approximately one radian there is an uncertainty in the reading which amounts to 20 min. 30 secs. What is the percentage error?

10. Identify the specimens *A*, *B*, *C*, ... *J*, and state what they are used for in a physical laboratory.

11. Determine directly the magnifying power of the simple convex lens provided. Compare your result with that to be expected on theoretical grounds.

Data: The focal length of the lens is 10 cm.

12. Weigh the given ball and return the weights to their places in the box.

13. Find the mass of the given metre stick by hanging it up. The weight and loop provided are to be assumed to total 200·00 gm.

14. Set the upper surface of the glass plate horizontal. Show your setting to the examiner.

15. Determine the period of oscillation of a wood rod when swinging as a pendulum.

16. Read the vernier and write down the result. The screw-adjustments of the instrument must not be touched.

A magnifying glass is provided. [Pulfrich refractometer.]

17. Write a list of the faults displayed by this apparatus for the measurement of the thermal conductivity of a copper bar as at present arranged.

[Searle's bar.]

18. State what this apparatus is ordinarily used for, and make a list of the faults shown in its present arrangement.

[Chorlton and Lees' thermal conductivity apparatus.]

19. Write your name on a scrap of paper ribbon. Cut off enough wire to make a circle 7 cm. in diameter, allowing for overlap. Push the wire through the paper label. Solder the wire ends together. Bend the wire to circular form. Drop it in this box.

20. There are three parallel lines A, B, C on the blackboard. Calling the distance AC one hundred units, estimate AB without measuring instruments.

21. What slovenly, non-conservative or anti-social acts have been committed here? (5 objects.)

[Hardened paintbrush, loose wire-end on a reel, stained weight, mercury in an aluminium cup, lens lying on emery cloth.]

(Spare.) Measure and record the radius of curvature of the given concave mirror.

Intermediate, 1927 *October*. 5-*minute experiments*

1. Determine the critical angle of the given liquid for sodium light.

[Total internal reflection by air film between parallel plates.]

2. If I find that the acceleration due to gravity is 935 cm. sec.$^{-2}$, what is my percentage error?

3. Set the spectrometer so as to show the continuous spectrum produced by the light of the electric lamp. Show the result to the examiner. N.B. Do not alter the focussing arrangements of the spectrometer.

4. Name this apparatus. Read the vernier.

[Fletcher's trolley.]

5. Measure the volume of the small glass vessel provided by running in water from a burette.

6. What is the difference between the frequencies of these two tuning forks.

[Method: counting beats with the aid of a stop-clock.]

7. Identify the three pieces of apparatus X, Y and Z.

[X, electrolysis apparatus; Y, Searle's conductivity bar; Z, apparatus to give a constant head of liquid.]

8. Find the diameter of the given ball.

[Micrometer screw-gauge.]

9. What is this instrument? Record the reading of the vernier.

[Sextant.]

10. Find the atmospheric pressure, using the barometer.

11. What are the given chemicals used for in a physics laboratory?
[Ether, lycopodium, Canada balsam, iron filings.]

12. What is this instrument called? For what purpose is it used?
[Moving coil mirror-galvanometer.]

13. What is this apparatus? Find the periodic time of vibration for small amplitudes.
[Kater's pendulum.]

14. Name the cells provided. Also state which are the positive and negative poles of each cell.
[Four cells: Weston, Leclanché, Daniell, accumulator.]

15. Solder two pieces of wire together. Write your name on a label, and stick the label on to the wire when you have finished.

16. Measure the thickness of the block of glass near the corner which has been chipped.
[Vernier slide calipers.]

17. A millivoltmeter and shunt are provided. What would be the reading of the pointer, if a current of 16 amperes was flowing through the combination?

18. What is wrong with this Post Office box, as set up to measure the resistance of the unknown coil of wire. (N.B. Do not alter any of the connections.)

19. State for each of the wires *A*, *B*, *C*, *D*, *E*: (i) the material of the core; (ii) the material of the covering, if any.

20. State which are the "refracting angles" of these two prisms. Which prism would you use if you wanted to obtain the spectrum of mercury, using the mercury vapour lamp and a spectrometer?
[One optical glass, one cheap glass.]

21. Find the specific gravity of the given liquid, using the common hydrometer.

22. Observe and record the time of a complete oscillation of the wood stick, using the stop-clock provided.

23. What is wrong with this apparatus?
[Dip circle.]

24. Add up the weights on the right-hand side of this balance. The case must not be opened.

25. What is the name of this piece of apparatus, and what is it used for?
[Hope's apparatus.]

26. Measure carefully the length of the bronze rod and write down your result.

27. Find the length of air column which resonates to the tuning fork provided.

28. *Do not touch this apparatus.* Is the point of the knitting-pin above, below, or at the centre of curvature of the glass dish?
[Glass dish acting as concave mirror.]

29. Read and write down the temperature of the water in the thermostat.

30. What is the name of this machine? For what purpose is it used? What indispensable parts are missing?
[Lathe, no belt.]

31. Stand in the chalk ring and look at the drawing-board. On a scale of lightness and darkness, call the black square 0, the white 100. What numbers do you then give to (*a*) the background; (*b*) the brown square; (*c*) the paler square?
[Results are reported in the *British Journal of Psychology* (*General Section*), **20**, 28 (1929).]

32. *Do not touch the apparatus!* Is the point of the black needle at the centre of curvature of the mirror? Or if not, is it set too high or too low? *Do not alter it!*

33. Find the volume of the piece of sealing wax. [Wax to be immersed in graduated jar containing water.]

34. Write down the reading of the vernier. *Do not touch the milled heads.*
[Pulfrich refractometer.]

35. Write your name on a gummed label. Cut off enough wire to make a circle 7 cm. in diam., allowing for overlap. Twist the ends together. Adjust to a circle. Attach label and drop wire into the box.

36. Record the reading of the spherometer when all four feet are touching the glass plate.

37. Estimate the position of the mark X on the scale of 0 to 100. Measuring, either with ruler or with pencil, is forbidden.
(Spare.) Read the two verniers on the cathetometer.

Final I, 1926 *March*. 30-*minute experiments*. *Two "vortices"*

1. Compare the e.m.f.'s of two batteries each of two cells. The connections of the vane must not be altered.
[Quadrant electrometer.]

2. Find the boiling point of ether by the Clowes *J* tube. *Caution*. The flame must be put out before the ether is poured. Hand tube to demonstrator at end.

3. Determine the radius of gyration of the block of metal provided. Do not remove the block of metal from its suspension. Compare your result with that obtained from measurements on the dimensions of the body.

4. What is the number of independent coordinates that must be known in order to specify completely the position of the following things: (1) Fletcher's trolley rolling on table; (2) wheel touching table at rim and at tip of axle; (3) ball rolling on dish; (4) balance free to swing; (5) india-rubber tube hung up?

5. Determine the density of the given sphere as accurately as you can. Indicate the order of accuracy you have attained, when writing up your results.
[Weighing sphere in air and water.]

6. Determine the logarithmic decrement of the ballistic galvanometer. Do not touch the shunt plugs.

Final I, 1927 *March*. 13-*minute experiments*

1. Adjust the spectrometer so that you can observe the separation of the *D*-lines of the sodium spectrum. Show the completed adjustment to the examiner.

2. Three mistakes have been made in setting up this Carey Foster bridge for measuring accurately the resistance of the coil marked No. 8831 by Pye and Co. State what these mistakes are. The plug is assumed to be connected to a 2-volt source of supply, and it *must not* be inserted into any socket.

3. What are the given chemicals for in a physics laboratory?
[Ammonium chloride, resin, ether, eosin, Canada balsam, platinum chloride, camphor, potassium iodide, acetone, aniline.]

4. Measure the accelerations of the given sphere and disc as they roll down the inclined plane. (N.B. The inclination of the plane is not to be altered by students.)
[Fletcher's trolley-bed.]

5. Cut the given piece of glass tubing in two, and join the two pieces together again, using the blowpipe flame. Round off the jagged ends, and stick a label on which your name has been written on your specimen. N.B. Only one tube is allowed for each student.

6. State what is wrong with this Rayleigh potentiometer, as it has been set up. (4 mistakes.)

7. Two prisms α and β, and two pieces of glass γ and δ are provided. Which of the prisms and which piece of glass would give the better definition when placed between the collimator and telescope of a spectrometer adjusted for parallel light? Give reasons for your answer. [Observation by unaided eye at nearly grazing incidence.]

8. The screws and clamps must *not* be altered, but stands may be lifted. Set the edge of the reflecting prism so that it coincides with its own image formed by reflection in the *further* surface of the simple lens. Measure the distance between image and reflecting surface by using the slide carrying a horizontal boxwood rod. Record your result.

9. Estimate the number of turns on the provided coil, being given the formula for a tangent galvanometer—(tan θ) is proportional to (turns) (radius)—and that the turns are 500 and the mean diameter 14·6 cm. The plugs may be altered, but *the wires must not be disconnected.*

10. A body is hung up by two threads so that it can make torsional oscillations about a vertical axis. For these find the ratio (period for an amplitude of one right angle) (period for a very small amplitude). Keep the plane of the ring horizontal.

11. A potentiometer will be seen connected ready for comparing an unknown with a standard cell. *Do not alter the wiring.* You may move the switch and the tapping keys. Observe and record the points of balance and work out the ratio (e.m.f. of unknown) (e.m.f. of standard cell). The potentiometer wire is soldered to the copper plates where it first touches them.

12. Measure the resistance and self-inductance of the given solenoid, using the Campbell variable standard. [Heaviside equal-ratio bridge arranged ready.]

13. *The adjustments must not be altered.* Read and record the settings of (*a*) Pulfrich vernier; (*b*) Vernier of slide calipers "insides"; (*c*) Vernier *on pillar* of travelling microscope (ignoring micrometer screw and its scale); (*d*) Vernier on pillar of cathetometer; (*e*) setting of micrometer screw on cathetometer.

14. Compare the two tuning forks. State which has the higher frequency and measure the difference of frequency. Provided: forks, rubber striking blocks, resonator, wax or plasticine, pendulum having a period of 2 seconds.

Final I, 1928 *March*. 15-*minute experiments*

1. Compare the e.m.f.'s of the two given cells, using the Rayleigh potentiometer. N.B. The connections have been set in adjustment, and must not be altered. The combined resistances in the two boxes must not be less than 1000 ohms.

2. Measure the accelerations parallel to the inclined plane of the given ball and disc. N.B. The inclination of the Fletcher's trolley is not to be altered.

3. Calculate the capacity between the two plates of the given condenser. State what type of error is made by assuming the aforesaid capacity to be the capacity of the upper plate.

[Parallel plate electrostatic condenser.]

4. Find the cooling constant for the given ball, i.e. (rate of cooling in ° C. per minute) (excess temperature of ball over surrounding air in ° C.).

5. Determine the period of small oscillation of the pendulum as accurately as you can, being given that the neon lamp flashes every two seconds exactly. The scale behind the pendulum is in centimetres.

6. Cut a length of the given wire from the coil, and make a circular ring of diameter 3 inches, soldering the two ends of the wire together. Write your name on a label and attach label to the wire. Drop completed circle into the tin provided.

7. *The adjustments must not be altered.* Read and record the settings of: (*a*) the Pulfrich vernier; (*b*) vernier of slide calipers "insides"; (*c*) vernier of sextant; (*d*) vernier on pillar of cathetometer; (*e*) setting of micrometer screw on cathetometer.

8. Find the sensitivity of the balance; that is to say, the number of milligrams per 1 division shift of the centre of oscillation. Do this: (*a*) for pans empty; (*b*) for a load of 100 gm. in each pan. The rider is 1 centigram.

9. Find, for rotation of the brass ring about a vertical axis, the ratio (period when the amplitude is 90°)/(period when the amplitude is small). The plane of the ring must be horizontal. [Bifilar.]

10. Two batteries are provided: *A* consists of two accumulators in series. *B* consists of some dry cells. Use its extreme terminals. Compare the electromotive force of *A* with that of *B*, by means of the quadrant electrometer. Wires must not be taken off the key.

11. Wires must not be altered on binding screws. Measure the resistance of the coil. Estimate the accuracy of the result. State the result in a form likely to be useful next session. What peculiarity do you notice in the coil? Take out *all* the plugs before you leave (marks for this). [A very inductive coil of copper.]

12. Saw from the rod enough ebonite to make a cylinder 2·5 cm. long and machine it exactly to that length in the lathe. The cylindrical surfaces need not be machined. Slide calipers provided. Hand finished work to invigilator. The invigilator will sharpen the tool when needed.

(Spare.) A telephone is emitting a note. The electrical connections of the telephone must not be altered. Determine by resonance the frequency of the vibration, with as much accuracy as time permits.

[Telephone fixed near mouth of a resonance tube.]

Final II, 1925 *December*. 8-*minute experiments*

1. Adjust the spectrometer so as to show the two yellow lines of sodium separate, and show them to the examiner.

2. The following data occur in an experiment for determining Young's modulus (E), where $E = MgL/lA$.

M = mass = 5000·3 gm.
l = extension of wire = 0·535 cm.
g = 981 cm./sec.^{2}
L = length of wire = 745·329 cm.
A = area of cross-section = 0·00703 sq. cm.

Obtain the result as accurately as possible, when you are told that the measurement of (l) is only accurate to 0·005 cm. What is the final accuracy of your result?

3. Adjust the lens, plate, reflector and telemicroscope so as to show Newton's rings, at nearly normal incidence, to the examiner.

4. Three mistakes have been made in setting up this Carey Foster bridge. When you have found them, write an account of them in your answer book. Do *not* alter any of the connections.

5. Determine accurately the weight of the sinker in the liquid together with the supporting wire. Record also the temperature of the liquid during the weighing.

6. Cut off a short length of wire from the two coils provided, and solder the two pieces together. Attach a label with your name on it, and give the wire, etc. to one of the examiners.

7. Decribe the correction for heat leakage in Callendar's apparatus. [*J* by friction.]

8. Find the electrical energy given out by the dynamo as a fraction of the electrical energy taken in by the motor, assuming that the voltage applied to the motor is 200. Do not alter the adjustment of the rheostats.

9. In an experiment on a flywheel, whose axle had a radius of 1·27 cm., a mass of 100 gm. was allowed to fall through a height of 141 cm. While the mass was falling, the flywheel made 16·5 revolutions, and after the mass had become detached, the wheel made 114·5 revolutions in 152 sec. before it came to rest. From the above data, calculate the moment of inertia of the flywheel.

10. Tune the 'cello to the fundamental or octave of the two forks supplied, but tell the examiner whether you mean it to be tuned to the fundamental or octave.

11. Cut the piece of glass tubing provided into two portions, and then join these two portions together again. Round off the sharp edges of the two ends in the flame. Write your name on the gummed label—stick the label on the tube—hand the tube to one of the examiners.

12. There are three mistakes in the connections of this Rayleigh potentiometer. Write an account of them in your answer books when you have found them. You are *not* to put them right or alter the connection in any way.

13. Determine the velocity of sound by means of the resonance tube and tuning fork provided.

14. Set the edge of the reflecting prism so that it coincides with its own image formed by reflection in the *further* surface of the simple lens. Write down the scale readings of the pointers of the two stands and subtract. Show to examiner.

15. Find the damping coefficient of this ballistic galvanometer.

16. Set the pillar of the cathetometer vertical. Show to examiner.

17. Identify the eight specimens *A*, *B*, *C*, *D*, *E*, *F*, *G*, *H*, and state what they are used for.

18. Compare the capacities of the two given condensers. (N.B. You are to put one condenser in circuit in place of the other, but must not alter any other connections.) [Method: ballistic galvanometer.]

19. Set the axis of rotation to pass through a nodal point, and a pin simultaneously to coincide with a principal focus. Write down scale reading of mark *A* on lens-slide. Show to examiner. [Compound optical system on turn-table.]

20. Balance the Kohlrausch bridge and write down the value of the tested resistance (marked 200).

21. Find out which of the four given capacities leak.
[Provided: gold leaf electroscope, electrostatic air-condensers.]

22. Determine the readings of the two sensitive thermometers at the transformation point of sodium sulphate. A common thermometer and warm and cold water are provided. State in writing any correction which you would have applied if you had more time. Do not heat bath above 50° C. The true value is 32·38° C.

(Spare.) Write a brief account of the correction for heat-leakage in Clément and Désormes' experiment.

Final II, 1927 *December*. 13-*minute experiments*

1. Adjust the spectrometer for parallel light by Schuster's method. Determine the angle between the bright green and the *bright* blue (violetish blue) line when each is in minimum deviation. End by showing the spectrometer to the invigilator.

2. Examine the refracting faces of the prism and report on their shape. Provided: large slab of glass flat on the average, mercury light, white light, tissue, benzene. Express your results with reference to the letters *ABC*, *DEF* at the corners of the prism. [Interference fringes.]

3. Cut off a suitable length of glass tubing, and make a "Clowes" boiling point tube. Stick a piece of paper with your name written on it on to the tube.

4. Identify the specimens *A*, *B*, ... *K*, and state what they are used for. [*Specimens set*: *A*, universal shunt; *B*, cutting diamond; *C*, CO_2 bomb; *D*, Leyden jar; *E*, part of Hibbert's balance; *F*, electromagnet with magnetic liquid; *G*, Edser and Butler's parallel plates and prism; *H*, neon lamp; *J*, Chorlton and Lees' apparatus for thermal conductivity; *K*, porous pot for diffusion experiments.]

5. Measure the acceleration of the trolley when acted upon by the given weight. (N.B. The weights are not to be altered.) Write your name on the trace you obtain and send it up in your answer book.

Assume the periodic time of the vibrator to be 1/5 sec., as stated on it.

6. Find the ratio of the rate-of-fall-of-temperature of the copper ball, to the excess of its temperature above that of the air. Do not go above 100° C.

7. Make a precise comparison of two weights each nominally of 100 gm. A centigram weight is provided. Refer to the large weights as "squat" and "tall." (Marks for method as well as for result.)

8. Determine the ratio of the frequencies of the two forks provided by means of the sonometer. Logs provided.

9. Determine the logarithmic decrement of the ballistic galvanometer on open circuit. The positions of the plugs must not be altered, nor the torsion head. Bring the needle to rest, using the given small magnet, and show the spot of light to the examiner.

10. Use the apparatus set up to verify Ohm's law. [Potentiometer-tangent galvanometer.]

11. Same as No. 12 for Final I, 1927 March.

12. Find for the optical system in the black tube, the distances of object and image from the ends of tube nearest to them; obtaining two or more such pairs. Do you notice any simple properties of the observations, if so, what? What is the name of any system possessing these properties? Show your setting to the invigilator when you have done with it. [Aldis unit sight; a telescopic system.]

13. Measure and record the deflections produced by the two cells separately when the quadrant electrometer is used idiostatically. Hence compare their e.m.f.'s. Log tables provided*.

(Spare.) This apparatus is supposed to be set up for measuring the resistance and sensitivity of the ballistic galvanometer. Find the mistakes which have been made in the circuit. (N.B. The second battery connection is *on no account* to be made.)

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* This led to the paper in *Proc. Phys. Soc.* **40**, 234 (1928).

Paper by J. H. BRINKWORTH, M.Sc., A.R.C.S.

Circulated before the meeting.

Physics is so essentially a subject in which numerical results are of the utmost importance that a good general knowledge of the science can only be gained when the course of study includes practical work of a quantitative nature. From the commencement of, and throughout, a course of instruction in practical physics emphasis must be laid on: (*a*) the importance of recording *all* experimental observations clearly, (*b*) the suitable variation of experimental conditions, (*c*) the avoidance of constant and systematic errors, (*d*) the estimation of, and statement of results to, the order of accuracy attained, (*e*) the use of common sense, (*f*) the application of a knowledge of theory. Towards the end of a course some instruction in manipulation and in the laboratory arts is desirable.

One function of a practical examination is to find out how much a candidate understands and remembers of a course defined, and generally limited, by some syllabus, in which he has received proper instruction. This is all an internal examination should be expected to do. An external examination should have an additional object in view, namely, the investigation of the teacher's method of instructing the candidate. The external examiner is very materially helped in gaining some knowledge of this if there is, and there always should be, an instruction that the candidate must present a properly accredited record of his practical work.

I think that the present-day examinations in practical physics are well designed to elicit all the information about the candidate and his course of instruction that one could reasonably expect to obtain in the relatively short time—3 to 12 hours—of a practical examination. This implies that I consider the general type of question set to be in accordance with the various syllabuses, that the latter are sufficiently comprehensive and well defined, that the time allowed for a practical examination is reasonable and that, in consequence, the candidate who has covered the prescribed syllabus will have no just cause for complaint. I do not think that candidates, taking any examination up to a pass degree standard, should have a choice of questions, for all questions in practical physics up to and including this standard—which is well defined—should be framed with the idea of testing the candidate's knowledge of fundamental physical measurements. Were choice allowed, difficulties in respect to the amount of apparatus available would certainly arise in dealing with large batches of candidates, if certain questions were more acceptable than others to the majority of them. However, in an honours examination, it is the general custom to ask the candidates to delete one or two questions out of ten, say, but the candidates are told that they will be asked to perform one of the remaining questions, only if it is practicable for the examiners to distribute the questions not rejected by the candidates.

The examiner must deal drastically with the candidate who cribs or cooks, but it is not practicable to prevent candidates from seeing each other's work. If, then, a candidate does "gather some roses while he may" and has common sense enough not to show how recently he has gathered his blooms, should the presiding examiner blame him?

I think all examiners realise (candidates and their teachers should realise this too) that their chief function is to *help* candidates to pass examinations. Such help is often most suitably given by the examiner asking the candidate a few leading questions.

It is quite a good scheme for the *teacher* occasionally to test students working for examinations up to intermediate standard, by giving all of them the same questions—such as eye-estimation of a length, the reading of a barometer, the determination of the focal length of a convex lens, or the spotting of an incorrect circuit, but I do not think that any number of such independent tests would enable an *outside examiner* to gauge the true merit of an examinee, and certainly he would not be able to form any opinion of the method of teaching other than that the student had been more or less well crammed.

For example, suppose the problem is "to determine the focal length of a convex lens." In the type of examination mentioned above the candidate might be allowed five minutes. His result would be, one rough value, obtained by focussing a distant object (though most candidates omit to do this) and one value calculated from a no-parallax setting with pins. On the present-day system a candidate would be allowed $1\frac{1}{2}$ hours; he should make a full investigation of the (u, v, f) relation, and he could be asked for certain graphed results. As there has been no choice of question, the examiner can confidently assume that the candidate who returns a good answer to the question, having been given sufficient time in the present-day type of examination, must have been properly taught and has undoubtedly benefited from his teaching, not only in optics but in other branches of physics.

The *School Science Review* of December, 1928, contains a symposium of the views of various teachers on examinations in practical science. Here is a quotation: "In the Higher School Examination the teacher must play for safety and his pupils must...perform the 'umpteen' variations on the Wheatstone bridge and potentiometer themes." Firstly, I wish that the writer had referred to the many *applications* of these themes (though the Wheatstone's bridge can be considered as a special application of the potentiometer theme), for such a wording would have emphasised the fact that the fundamental measurement is always the same, i.e. a potential difference. Secondly, I would ask if the words "playing for safety" indicate that more emphasis is laid on the many variations, or applications, rather than on the fundamental principle involved?

Suppose a candidate is asked to determine the specific resistance of a material in the form of a wire. How often will he show evidence that, during his course of instruction, sufficient emphasis has been laid on the points mentioned above? For instance, on (*a*) by recording several measurements of the diameter of the wire; on (*b*) by measuring the resistance of different lengths; on (*c*) by interchanging the comparison resistance and the given wire; on (*d*) by recording shift of contact which will give a deflection first one way, then the other—see also (*a*) above; on (*e*) by uncoiling the given hank of wire (generally bare); and on (*f*) by recognising that the wiring diagram would have been the same if a Post Office box were used instead of the bridge.

The examiner can generally decide whether the candidate's sins of omission are due to nervousness or to forgetfulness: he can, and will, often prompt the candidate, but the latter will recognise that help is being given, only if he has been trained to make physical measurements properly.

PAPER BY PROF. W. WILSON, PH.D., D.SC., F.R.S.

Circulated before the meeting.

THE purpose of an examination is, or should be, to test with what success a student has followed a course of study and this means, in the special case of practical physics, to test his acquaintance with the experimental side of physical investigation. It is unfortunate that examinations have become an end in themselves. It is the chief goal of many or most students to pass examinations, and the efforts of teachers are too much directed to drilling their pupils in methods of meeting examination questions, when they should be devoted primarily to giving them an intelligent grasp of the subject. Examinations are not in themselves very evil institutions; but they bring a multitude of evils in their train.

My experience of practical examinations in physics is chiefly derived from the London External Intermediate and Higher School examinations and I am convinced that, in these examinations at all events, the practical part is distinctly more reliable than the paper examination, owing largely, no doubt, to the contact between the candidate and the examiner. A paper examination has always the weakness that no subject about which the candidate writes in an unsatisfactory way can be followed up, and the examiner is often in doubt about the value of what he reads. In a practical examination he has it in his power in some degree to resolve any doubts.

The syllabus should define the scope of the examination in broad outline only, and should not be minutely detailed. A detailed syllabus encourages an objectionable kind of parsimony and also misleads the candidate.

The exercises usually set in practical examinations at the present time seem to me to be of the right type. They mostly consist in the determination of physical quantities, e.g. thermal conductivity, intensities of magnetic fields, focal lengths and so forth, or in the completion of some short physical investigation such as the dependence of the frequency of vibration of a wire on the stretching force.

As far as possible the exercise should require the candidate to devise expedients and to think out how best to utilise the apparatus at his disposal. It should test his accuracy and ability to obtain observations or data, as well as his ability to make the best use of them and to eliminate, or estimate and allow for, systematic errors. Ample time should be allowed for the exercises.

It is quite common to allot only two exercises to the candidate and I get the impression that this is thought to furnish too narrow a basis on which to assess his merits. A candidate may apparently have the misfortune to be judged by one type of exercise he has never done and so suffer injustice. This kind of apprehension is not well founded. Nobody is better aware than the examiner how unsafe it is to judge a candidate on his failure to do the particular exercise allotted to him. The examiner can at his discretion give an alternative exercise; and he should have, and usually has, at his disposal a very great variety of exercises. He may, and often does, give assistance to the candidate, and it is an extremely unlikely contingency that a reasonably experienced examiner will fail to estimate with considerable accuracy the merits or demerits of a candidate, notwithstanding the apparently narrow basis of two exercises.

No right of choice should be given. The examiner should allot the exercises, but he should be at liberty to allow an individual candidate to choose if he thinks this desirable, as for example in the case of a weak candidate.

A new suggestion for practical examinations, evoked, as I understand, by the apparent narrowness of the present examinations, has been made by Dr Richardson and Mr Maxwell. It is proposed that the candidate should carry out a large number of short elementary exercises of every variety, each to be completed in a very short time, perhaps in 10 minutes. I cannot help thinking that such an examination misses the main thing. In not a few of the experimental investigations that have contributed appreciably to the advancement of physics the observations have been made by a laboratory assistant who, with no grasp of the science at all, was nevertheless trained to take accurate readings. The practical examination should test, among other things, to what extent the candidate has advanced along the road to becoming an experimental physicist. This new type of examination is calculated, I think, to injure the teaching of laboratory work. It is desirable that the student should take a delight in carrying out physical experiments. Some of the most eminent experimental physicists have owed their success in no small measure to the aesthetic satisfaction experienced in carrying out measurements involving a long train of observations or the use of some ingeniously devised apparatus. The suggested type of examination would cause attention to be directed mainly to details like vernier readings, adjustment of spectrometers and so on, and the far more important things which are to be attained by the aid of such elementary operations would be lost sight of, and the student's interest and satisfaction with his work would suffer.

Before closing this rather meagre contribution to the subject of practical examinations, I cannot help referring to one of the headings proposed for this discussion, namely the problem of the help derived by the candidate from seeing another doing the same experiment. It is not a formidable problem at all. If a candidate is so good a physicist that he is able, by the scanty observation of someone several yards away, to seize the ideas involved in the exercise allotted to him and so bring it to a successful conclusion, he should certainly pass.

GENERAL DISCUSSION

(*Abridged report.*)

Mr J. H. BRINKWORTH, in presenting his paper, exhibited a large number of diagrams of a metre bridge, handed in by candidates at a recent examination. The diagrams were generally incorrect and indicated, in the speaker's view, failure on the part of the teachers to relate the symmetrical diagram of the Wheatstone net with its embodiment in laboratory apparatus.

Mr Brinkworth then proceeded as follows: Quite certainly, the only thing for which we, as teachers, should be able to blame our students is forgetfulness. In my written contribution to this discussion I have expressed the view that the present-day examinations in practical physics (I speak only of University of London examinations of Higher School, Intermediate and Final standards) are well designed, especially so because the examinee is given sufficient time to carry out an experiment properly. Thus, in 1½ hours (which should be the minimum time allowable) a candidate can perform the Wheatstone's bridge experiment to which I have referred and he will be able to pay attention to the various points mentioned. Should he forget, judicious prompting by the examiner will help him to remember if, and only if, he has been taught to recognise the importance of these several measurements.

I feel that all external examiners must criticise the pantopiric method adversely because it does not allow time for an examinee to show that he has been taught, and that he knows how to carry out, a physical experiment properly. If I am right in thinking that the main object of a practical examination is to elicit such information, then my criticism of the new method can be indicated by the suggestion that the new adjective, when used before the word "examination," should be spelt "pantopyrrhic"; for this homonym may surely be taken to mean "that which completely defeats its own ends." I would like even the most junior students to be taught to carry out every experiment as if it were a piece of research. As only a limited amount of time is available in a school for the study of physics, this may necessitate curtailment of the syllabus of practical work suitable for such students. But, if I may still harp, I fear rather monotonously, on the single wire theme, I would emphasise that it is, in my opinion, better for the elementary student to try to master this important experiment than it is for him to attempt to be Jack of all trades and master of none.

Dr A. W. BARTON (Repton). As I am the first member of the Science Masters' Association to speak, it is perhaps appropriate that I should say how much we appreciate, as an association, the privilege of joining in this discussion by the invitation of the Physical Society. Having been asked to confine my remarks to elementary examinations of the School Certificate standard, I should like to emphasise in the first place that such examinations are designed to test the general education of the candidates, not their ability as specialists in physics. Nevertheless I am strongly in favour of the retention of practical examinations at this stage for the following reason. The candidates spend some 20 out of the 24 hours of the whole examination in *writing* about things; it is only in the 4 hours' practical examination that they are required to show how well they can actually *do* things. The ability to do things is perhaps of greater value in after life than the ability to write about how to do them, and it is the practical examination which tests this "doing faculty."

The examination should test the candidate's ability to make careful and precise observations and to carry out accurate and reliable measurements. He should also be required to deduce any logical conclusions that may follow from his experimental results and be able to set out the whole of his work clearly and concisely. The latter point is most important, as complaints are continually being made that there are so many people who have something to say but cannot say it intelligibly. The Rector of the Imperial College mentioned only yesterday that he had come across cases of research students, working for the Department of Scientific and Industrial Research, who had to be turned away because the reports of their work were incomprehensible to their supervisors! The Oxford and Cambridge Joint Board School Certificate papers—the only ones of which I have any experience—do satisfy these criteria.

I feel that a comprehensive syllabus of the practical work on which the candidate will be examined should be supplied, but that it should not be unnecessarily detailed. I am not in favour of a scheme whereby a list of experiments which the candidate is expected to know is supplied, the examiner being forbidden to set any experiment not on the list; on the contrary I welcome the type of question that involves an experiment which the candidate will not have done in the course of his preparation for the examination, and even an experiment of which he does not know the theory. But I should like to enter a strong plea that such questions be worded with the greatest care so that the instructions may not be likely to be misunderstood. I may perhaps be allowed to illustrate this point by a question which we had at Repton last July; "Compare by a potentiometer the e.m.f. of the given cell with the p.d. between its terminals when *delivering a current through* the resistance A." I submit that the words *delivering a current through* should have been replaced by *short-circuited by*; as it was, quite three-quarters of the candidates placed the resistance A between the given cell and the galvanometer! It is easy to say that they should have known better; but I feel that it is expecting too much of boys to wrestle with a somewhat strange apparatus involving three circuits, especially when one bears in mind the trying conditions of an examination and their lack of discretion.

We now come to the matter of choice of questions and the time to be allowed for the examination. I believe that the fairest method is to have no choice of questions, as is the case at present. But I should like to see the duration of the examination extended from 2 to $2\frac{1}{2}$ hours, as I am sure that a good candidate is unduly hurried at present.

The Oxford and Cambridge Joint Board conduct their School Certificate practical examinations without the presence of an external examiner. This is surely unsatisfactory; if an external examiner had been present last July he could have helped the candidates in the question criticised above had he seen fit to do so. He would then have seen whether they could get an accurate result, and whether they knew what to do, but actually the examiner had little or no material on which he could estimate the candidates' worth with regard to that question. There are, of course, grave difficulties in the way of supplying examiners for such a large examination, but I hope that some attempt will be made in the near future.

Lastly I have been very interested in the "pantopiric method" described by Dr Richardson and Mr Maxwell. I should not like to express a definite opinion about it as yet, but I feel that there are possibilities in it and that it is likely to be more successful in elementary examinations than in those of a more advanced type.

To sum up, it is my opinion that elementary practical examinations serve their purpose reasonably well. Certain improvements, such as those I have indicated, might well be made. But, on the whole, the abler candidates do come out at the top and the weaker ones at the bottom. And I believe that the practical examination influences the result to just the right extent; it is only a very bad mark in the practical examination which causes a candidate who has done a good theoretical paper to lose his "credit," and he deserves to! Conversely a candidate who does a moderate theoretical paper often gets a "credit" by doing a very good practical paper, and he deserves it also.

Mr F. A. MEIER (Rugby). The efficacy of any examination or system of examinations is judged by results. If by this means a large number of boys can be graded according to their merits in a particular subject, the system may be said to be justified by its results. However much we may condemn examinations, we are compelled to admit that with few exceptions this is secured by the present system, at any rate in theoretical subjects. But what I have just said does not apply to practical examinations. I am very doubtful myself as to what faculties a practical examination does test. But whatever it tests, only the very sanguine would maintain that it successfully grades a number of boys according to their practical ability.

After an experience of nearly 20 years in preparing boys for Oxford and Cambridge Entrance Scholarships and for Higher Certificate Joint Board, I feel it is still largely a matter of luck whether a boy who is proficient in experimental physics will do well in the practical examination. Now this is obviously not a state of affairs that should be permanently tolerated, and the question to be decided is whether this is inevitable in any practical examination or whether the present system could be so modified as to secure more satisfactory results.

I shall confine my remarks to the examinations just mentioned, as I can speak about them from personal experience, and I take it that they are typical of practical examinations all over the country. What should advanced practical examinations test, and what do they test?

The following is a quotation from a practical physics paper set at Cambridge many years ago: "Candidates will be expected to send up a short statement of the method they intend to employ for each of the 8 experiments together with the measurements they intend to make. No candidate will be expected to attempt the practical work of more than 3 questions."

Here a boy was confronted simultaneously with 8 problems. He had—at a moment's notice—to think out and put down the methods he intended to adopt; and not only that, but to state the measurements he proposed to make. He was acutely aware that he had only 3 hours at his disposal and that every minute was precious. Not only had he to write 8 accounts, but he was compelled to make a choice of the 3 questions which he finally selected. In doing so he undoubtedly often chose those which he was least competent to deal with. Happily the situation has improved greatly since those days, though the element of time is still a factor of too great importance.

What are the qualities that enable the physicist successfully to discover the laws of Nature and to enlarge the boundaries of scientific knowledge? Surely they are imagination, originality, initiative, power of concentration and perseverance, the ability to observe carefully and to measure accurately. In fine—do they not include the power to draw the correct inferences and to present them in such a way as to be understood and appreciated by others? I maintain that in discussing practical examinations we should have these qualities in mind as an ideal.

I feel very strongly that in advanced examinations the element of hurry should as far as possible be eliminated, so that an imaginative boy, who is influenced most by an atmosphere of bustle and haste, may be given a chance of bringing his mind to bear in peace on one problem at a time. He should be encouraged to observe carefully and to measure accurately, and should receive credit for so doing. The physicist is always on the look-out for alternative methods for testing the results of his work and is never content with an isolated result; so in a practical examination opportunity should if possible be given for obtaining the result by more than one method. No candidate should rest satisfied with a number of isolated numerical results which are the answers to a set of different problems.

I would therefore put in a special plea for *accuracy* of observation and measurement. And this leads me to the very heart of the matter. If a candidate is to be assessed for the accuracy of his work, it is obviously necessary to know to the required degree of accuracy the answers to the various problems set. But examiners cannot spend laborious days and nights determining the constants of lenses, reduction factors of galvanometers, resistances of cells, moments of inertia, etc. to the degree of accuracy required. Possibly they shelve the difficulty by asking the master in attendance to provide all the relevant and necessary information. But this has to be obtained on the day of the examination, and the master whose lot it is to obtain it does not feel great confidence in results produced, as they are, in a hurry. If he is wise, he will allow a reasonable margin for his own errors and will send in, for example, the focal length of a concave lens as 30 ± 1 cm. instead of giving it to the nearest millimetre. This effectively prevents the examiner from marking for accuracy. Supposing even for a moment that the solutions are known to the required degree of accuracy, it is still necessary, if justice is to be done, to assess the relative degree of accuracy for each experiment requiring the same amount of skill from the candidate. This is a far harder problem for the examiner and along this line I can see little hope.

Yet another difficulty confronts him. An experiment on magnetic moments is as fundamentally different from one on optics or heat as the scent of a rose is from that of a violet; and yet he is asked to make a numerical comparison.

In offering a solution to the difficulties just raised without sacrificing any of the principles laid down as essential, I shall deal at once with the syllabus, type of questions set, choice of questions, and time allowed.

I should like to see one question only set in the advanced examination without any choice being given. The scope of the question may be wide but if it is of an unusual kind it should be accompanied by very full instructions. The time allowed should be ample so that the candidate may not be hustled, and he should not be required to hand in his paper on the stroke of the clock, if he still has important measurements to take on which the final result depends. Possibly he should be slightly penalised for overtime.

This to my mind would solve the chief difficulty, and in large measure overcome the unsatisfactory nature of the present tests. In brief the result would be that: (1) The boy would be able to concentrate his attention on one problem at a time and give all his powers to careful and accurate observation and measurement. (2) The sense of rush engendered by the simultaneous presentation of a number of practical problems involving the visualization of a large number of experiments would be eliminated. (3) There would be no necessity for making a rapid choice in a state of excitement. (4) There would be time and opportunity for devising alternative methods of solving the problem set.

From the examiner's point of view: (5) He would no longer be compelled to compare the incomparable. (6) He would be able, at a small sacrifice of time, to find out accurately the solution to the problem and become acquainted with the practical difficulties from first-hand experience. He would therefore know the degree of accuracy that could be expected in the time available. (7) He would no longer feel any qualms of conscience in marking highly for accuracy and no uncertainty in his mind as to the justice of his marking.

Already there are signs of a change. The universities very often set two problems which are to be done in 3 hours, and give no choice. I hope that in time the number will still further be reduced to one. There are of course practical difficulties in the way of such a reform. If all the candidates are to do the same experiment there is the problem of apparatus to be considered. The equipment of the university is available for the Entrance Scholarship examination but the authorities responsible for the Higher Certificate practical examination have to be content with what can be supplied at the various centres at which the examination is held. But even so I think that something might be done to reduce the present element of luck. If three questions were set, two of which were allotted to each candidate, it would at any rate give the examiner a better chance of making a fair comparison of the work of the candidates. Needless to say I would like to know that it was obligatory for the examiner who sets the questions to have performed all the experiments himself shortly before the paper is finally set.

As far as I know, examiners are always present at advanced practical examinations. Whether they obtain all the information they might by so being present is a debateable question that would lead us too far afield.

With regard to the problem of help gained by a candidate by seeing the other candidates at work, I feel certain that material help is often obtained in this way.

I should like to suggest that no practical work be started until a short written account of the method and the measurements each candidate proposes to take has been given by him to the examiner, together with a diagram of any required electrical circuit, optical arrangements or the like. Possibly it might be of assistance to the candidate to have before him, whilst writing the description, the actual apparatus he is intended to use. Such an account should be marked 20 or 25 per cent. of the maximum for the examination. I would even recommend that if a candidate be unable to deal with all the theory, he should receive help from the examiner and forfeit all or most of the marks allowed for that portion of the paper: but that he should not be penalised further in carrying out the practical work. I have tried the method of requiring a preliminary account on various occasions recently, and am of the opinion that it is a partial solution of the difficulty. For example, in using a voltmeter a boy connected it up quite wrongly in his preliminary diagram but in actually using it he made the correct connections, having either realised his own mistake or seen the work of a boy near him. By the new method such a boy would be penalised, whilst by the old method his mistake would pass unnoticed. Of course experimental details could be handed in with the rest of the work at the end of the examination, and only where a radical change of method had been adopted would it be notifiable to the presiding examiner.

In conclusion I am sure that practical examinations should be *practical* examinations and should not depend on ability to solve a mere theoretical conundrum. What could be more unsatisfactory than a question like the following: "Find the time of oscillation of the given rigid pendulum about two different axes. Hence deduce a value for g"? The whole of the practical work involved is merely to find a time of swing of a pendulum and the rest is unadulterated theory. This is only one example of many. On learning of the "pantopiric method" which has been described to-night I decided that I would try the experiment at Rugby so as to be able to speak with experience about it. I must say I was fascinated by the paper, and equally so by the long list of experiments given at the end of it, and I intend to borrow copiously from them in future.

I tested 60 boys in two ways, giving them first six experiments in 1½ hours, and then one experiment in 1½ hours. I was amazed at the confidence I felt in marking for the results of these short experiments. It was really perfectly wonderful to feel that I really knew that those boys whom I brought out at the top deserved to be there. I had never felt that before.

But I am not so sure what those 15 minutes test. They do test something, and test it with certainty; but I do not feel certain in my own mind that they test those qualities in a student who is studying advanced physics that they ought to test, and I think the ideal would be a combination of the two methods: a 3 hours' paper of the short experiments, which would test technique and so on, and a 3 hours' question without choice. If we combined those two, we should come pretty near to an ideal.

Mr E. R. THOMAS (Newcastle). May I have the opportunity of giving evidence of the experience of the presence of an examiner in the School Certificate practical examination viva physics and chemistry. That has been the practice for some years, and it is the experience of all that the amount of collaboration, the amount of co-ordination between the work of the schools and the university, what the teachers have learnt from the examiners, and what the examiners have learnt from the schools in that time, the mutual confidence and respect which has grown up, is definite evidence that it is well worth while considering whether some of these very large examinations might not be broken up, or whether some means could be arranged whereby in every School Certificate practical science examination there shall be an accredited examiner present.

Mr E. NIGHTINGALE (St Albans). I should just like to make a few remarks on the first paper. First of all, from the point of view of the schools. If we could have, say, 20 experiments set up, 9 minutes to devote to each, and 20 examiners in order to check the results of those experiments, it might be rather interesting to see what would happen. As far as I could make out, you must do the checking as you go along, which necessitates a large number of examiners. We have been trying for years to get examiners to come and see what is being done in the schools during the practical examinations.

My criticism of the method is that it sorts out only the quick candidates: the fellow who can set up a spectrometer and adjustment in 9 minutes is a genius. The best work in my opinion is not done in physics by the rushers. That brings me to the point that I think it is a great mistake even in honours examinations to expect too much from the candidates. Very often those who get the first class honours are not necessarily the best physicists in the long run. Really good work is only done slowly, and people like Newton, Boyle and Cavendish all had ample time to think about what they were doing. They were not working in a rush. That is a point well worth bearing in mind. Sometimes I wonder how Faraday with his limited mathematical ability would have got on if he had had to take a university degree. I am afraid he would not have taken one!

Mr L. OULTON (Abertillery). We ought to bear in mind that 90 per cent., perhaps we may say 95 per cent., of the pupils who are being examined will not go on any further with science; they will go out into the world. We are inclined to speak as if we were going to test the ability of potential scientists or potential physicists only. I wonder if we sufficiently bear in mind that in a very large number of schools the practical examinations have an almost overwhelming influence on the type of education which is given in science to these 95 per cent. of pupils who will never go on any further after the School Certificate stage.

Mr V. T. Saunders (Uppingham). I take it we are all agreed that we require practical examinations. Then we take the marks of these practical examinations and add them to the marks that we get from written papers. I am not quite sure (I think this is a point that we really need to state in discussing this matter) that we are in fact measuring two things which can be measured in the same units. I personally doubt very much whether anything is gained by adding the marks in a practical examination to those marks that we get from a written examination. That is a point which seems to me to need consideration.

If I may return for a moment to the pantopiric examination, I am totally unable to understand how you can get a correlation of 1·3°.

Mr R. S. Maxwell. It is because the standard error is so high. The correlation is 1·19. The standard error is 1·06. That is too great, as is mentioned in the paper. Of course, it cannot really be valid.

Mr V. T. Saunders. It seems to me that the distribution of marks which has arisen from the pantopiric method is totally abnormal. I cannot feel much confidence in a method of examination which seems to lead to the conclusion that a great number of candidates were excellent, a greater number were bad, and that a medium number of marks were scattered irregularly through the middle zones. I think we should like to hear something more about a method that leads to that distribution.

Mr I. Williams (University of Bristol). I am strongly opposed to practical examinations in any shape or form. My objection to these examinations is based on the following considerations: (i) The practical examination, as usually conducted, is very unfair and untrustworthy; for example, one candidate *A* may be given an experiment involving considerable manipulation while a second candidate *B* may have to make a few simple readings on the scale of an instrument. (ii) Candidates are taken away from their own laboratories, where they feel at home, and are examined in some other laboratory where everything is unfamiliar. This objection is not diminished in the slightest degree by the suggestions put forward this evening. (iii) The candidate who comes from a well-equipped school in a large town stands a much better chance in a practical examination than an equally good candidate who has been educated at a small country school. (iv) Most of the people who favour these examinations admit quite freely that candidates do get suggestions from seeing the work of others; this connivance at cheating seems to me a very disagreeable feature in connection with these examinations. The very people who wink at cheating in a practical examination would be the first to condemn anything of the sort in connection with an examination in theoretical physics. (v) The practical examination tends to encourage cramming. The proposals of Dr Richardson and Mr Maxwell will, I think, only make matters worse. The poor unfortunate candidates, for several weeks before the examination, will be cogitating over such fundamental problems as the following: (*a*) "Can you identify the Richardson silk band?" (*b*) "Do you recognise the Maxwell conductivity bar?"

Having stated some of my objections to these examinations, I will indicate briefly what I propose as a substitute: (i) Let the teacher in charge of physics give the boys as much experimental work as is possible within the limits of the money at his disposal for the purchase of apparatus. (ii) Let the teacher supply to the examiners a report on the experimental work of each candidate. I feel confident that if the teachers were allowed to work on the lines which I have indicated above they would get excellent results.

Mr H. ROTTENBURG. I cannot help feeling that the authors of the pantopiric type of examination have not gone on to the logical conclusion of their train of argument. Some years ago I examined in applied physics at Oundle School, and as a result my view was that practical examinations were not satisfactory as ordinarily set; this led me to analyse the qualities of candidates that are tested by a practical examination. I came to the conclusion that practical examinations test: (*a*) the candidate's knowledge of how the experiment should be conducted; (*b*) his physical dexterity of eye, ear and hand; (*c*) his genius or capacity for taking infinite pains with details; (*d*) his inventiveness or power to strike off suddenly in a new direction.

(*a*) can be tested by a written examination though it cannot be learnt properly except by practical work, especially as regards such points as the precautions to be taken to ensure accuracy. (*c*) may actually tell against the candidate by prompting him to strive for specially accurate results which it is beyond the power of the examiner to assess justly in marking. It will in many cases prompt a boy to spend far too much time, as judged by mark-getting results, in trying to adjust an instrument which may be out of adjustment. I have certainly found this so. (*d*) can be tested to some extent by written papers but in practical examinations the nerve tension will deter most candidates from giving rein to it, and the adventurer type may succeed inordinately or fail ignominiously. (*c*) and (*d*) are mutually antagonistic and the judgment to know when to allow qualities (*c*) or (*d*) to guide one is too difficult for examination work.

This leaves (*b*), which can be perfectly tested by a series of individual tests of which the setting of a vernier is merely one. The suggestion discussed with the late Dr Sanderson was to examine classes of, say, 24 boys at a time, giving each 24 tests for perfection of co-ordination between hand, ear, eye and brain. The tests were to take, say, 3 minutes each with intervals of 2 minutes between. Each test was to be so designed as to eliminate inaccuracy or want of adjustment of the instruments. For instance, where a rotating pointer was timed by a stop-watch the watch and pointer were driven by the same mechanism, so that there could be no disagreement between them. A number of similar experiments was actually designed. The advantage of such a system of tests is that it definitely tests only physical as distinct from purely mental excellencies. The tests can be applied to a boy or man at different ages and give him an idea of the progress of the education of his hand, ear and eye resulting from the doing of experiments. They show also the relative excellence of different classes in a school. Many will say that it would lead to boys' practising soulless exercises. The answer to this is that soulless exercises of that sort are what make the great singer or musician; and the superlative excellence of the American golfers in matches against British golfers is almost universally put down to their will to practice soulless operations like putting, etc. The fact is that the will power to concentrate on soulless exercises is what makes the real genius, while the possibility that they may kill the future researcher is answered by remembering that they certainly do eliminate the superficially-minded researcher who has not the determination or incentive to persevere in that direction.

As regards the evil of teaching practical physics with one eye on examinations—is this not a natural reaction of teachers to the evil of setting catch questions?

Mr C. G. VERNON (Bedales). May I emphasise the importance of having an examiner present? I personally have been disgusted with the discrepancy between what I know to be the ability of some pupils and the marks obtained. Otherwise the practical examination chiefly tests the boys' ability to write an account of how they ought to be able to do things.

Mr A. G. COOMBS (Barnard Castle). I should like to ask, in connection with this pantopiric method of examination, if there was any attempt at setting the candidates in order according to the estimation of the teacher before the pantopiric method was applied. Every master knows in his own mind the order of merit, and I wondered whether the correlation between the estimation of the teacher and the pantopiric results had been worked out? A great deal of the difficulty of the practical examination could be overcome by asking the master to furnish the examiner with a list of the candidates in order of merit.

Mr DUNGEY LUNN (Birmingham). I happen to be a member of an examining body which is examining the largest number of candidates in this country above the School Certificate and the Higher School Certificate. I think the vital point in a practical examination, if it is to be worth while at all—almost an essential point, as has been emphasised by one or two speakers this afternoon—is that it is absolutely necessary that the examiner should be present. There are 14,000 candidates examined by the board to which I refer in the Northern Universities. 8000 of those candidates took physics this year. Would you kindly inform me how any scheme whatever is to be devised whereby an examiner can visit some 600 schools and examine 7000 candidates personally?

For that reason, and for that reason only, I am entirely opposed to a practical examination at the School Certificate stage. It is an ideal but it cannot be made practicable. The same difficulty occurs at the higher stage, and the only solution of the difficulty at the higher stage is being met because the examining body, the Joint Board, tests implicitly the physics teachers in the schools which it examines, and the degree of correlation between the estimate of the master teaching the subject and the results of the examination is exceedingly high, about 70 per cent. this year. Every master sends an estimate of the qualities of his candidates. He arranges them in order of merit, and the results are correlated. But I ought to say that so far as science is concerned, the estimate is on the subject as a whole. There is no separate estimate for practical and theoretical examinations.

With regard to Mr Thomas's point about splitting up the examinations, the Examinations Council is already in very serious difficulties: it is attempting to correlate the results of eight examining bodies and it finds very considerable difficulty in securing that the standard of the examination shall be the same throughout the eight bodies. This is an important point, because there are 54,000 candidates for the School Certificate. All who are successful are going to be hall-marked with the School Certificate, and we are aiming at making the standard the same for the whole country, but we are confronted with the very serious difficulty of correlating the Joint Board examination closely with the examinations held by all the other bodies.

Mr C. J. WILTSHIRE (Cardiff). Like the previous speaker I do not think practical examinations are advisable at the School Certificate stage. In our school we should have 70 boys going in for a practical examination. We have not enough accommodation. But for the higher examination, I think it is most desirable that a practical test should be held. I am sure also that most of us welcome the presence of an outside examiner; we benefit very considerably, and I think the examiner does so too.

At the end of the examination last year I asked one of the examiners, "What is the use of these note-books? What part do they play in assessing the marks?" He replied, "We test the defeated by the note-books." I think that is a very good point, because if a boy has a note-book kept up to date and carefully written, with diagrams and so on, it is a criterion at any rate of his work during the term and I think he should be given a fair percentage of the marks on his note-book. Of course, there is the difficulty that a boy may write up very nice things copied from a card, but at the same time the examiner can judge pretty well what he has been doing during the year, especially if he is compared with other boys in the same class.

Mr E. R. THOMAS. Examiners can be found. Are they not found for oral examinations in French? The time required would not be any longer. If they are not found for oral examinations, they should be. Moreover the advantage to the schools far outweighs even the great importance of the accuracy of the examination results.

Mr W. F. HARLING (Marlborough). We have been told that candidates can be judged after a quarter of an hour's examination, and also it seems to me that we might for once take a hint out of the note-book of the Army. I refer to the Certificate A examination, in which there is a practical test and a theoretical test. The practical test is purely a qualifying one. The boy passes, or he does not; if he does, he takes the theory examination.

It seems to me that the same system might be adopted in physics examinations. The boys should take the practical examination in the presence of an external examiner. These gentlemen are extremely skilled in assessing practical ability at short notice, and after half an hour they will be perfectly clear what boys have the practical ability to entitle them to spend time on the theory.

Mr J. C. PLATT (Salford). It seems to me we have heard a very great deal about the examination but very little about the candidates. They are human beings. At first they are a bundle of nerves. For the first quarter of an hour they have not settled down at all. How can you apply the suggestion of Dr Richardson and Mr Maxwell in a practical examination? For the first 10 minutes they will do nothing at all. They are excited. They will grow worse and worse under that scheme.

Mr F. TWYMAN. In reading accounts of experiments, I always find one thing that troubles me, and that is ambiguity. It is almost universal. One may read scientific papers by the greatest exponents and sometimes be in doubt whether they mean one thing or another. Telling a truth in a way that cannot be mistaken is a thing that cannot be done in a short time, and I think that for the writing up of results of a practical examination practically no time limit should be set. Unlimited time should be given.

Mr S. T. E. DARK (Strand School). We have tried the pantopiric method at Strand School for a small number of Higher School candidates in their first year and have found it very successful indeed. It does test quite a different thing from the ordinary examination. On the other hand you want to test whether a boy has done certain experiments. If he has done them, he ought to be able to set them up quickly and get results quickly. It seems to me that the ideal plan is to combine the two methods. We had the same thing in our theoretical papers. One was a test of pure memory work, and curiously enough we found that the same boys came out well in all these methods. But the ideal plan is to combine the methods and add the results together.

Prof. J. C. PHILIP (Chairman). I will ask those who opened the discussion to reply briefly.

Mr F. A. MEIER. The pantopiric method does not require a large number of examiners. I think one examiner can perfectly well look after 20 candidates and test the accuracy of their results as he goes along. I find 18 about the maximum number that can be handled.

Dr A. W. BARTON. May I refer to two points. One is the question of schools where there are large numbers of boys taking the certificate. We often have a large number compared to our size, 45 boys, taking the certificate at any one time out of a school of 420. What we do is to split them into two batches. We have the first batch in, complete the examination, then lock them up and get the next batch started, and then let the lions out.

Secondly, I would like to refer again to the question I quoted about a potentiometer. I admit that the boys ought to have known that it was no use putting the resistance between the battery and the galvanometer, but the point is that boys do not know those things when questions are sprung on them under examination conditions. Five minutes later they all knew what they had done wrong. I do feel that in a question of that sort, which is just a novelty to them, a clear instruction might have been given. Too much was expected of them under examination conditions at that early age.

Mr J. H. Brinkworth. I remember saying that the only thing we should blame the candidates for is forgetfulness, and Dr Barton has just made that point quite clear. When I said that I thought the present-day examinations were quite good, I was referring only to examinations of Higher School and standard upwards. I know nothing about the School Certificate type of examination. I am very sorry to hear that two candidates are expected to do two questions in 2 hours. That certainly is insufficient time. With regard to the Wheatstone bridge and the network, I am very pleased indeed to hear that everyone teaches the network at the same time that they teach the potentiometer. What I did was to bring concrete evidence that there are hundreds of candidates who do not know that the Wheatstone bridge and the network are the same thing. I think that the Wheatstone bridge experiment, to take that as an example, should be taught even to the most elementary student, that he should be taught to carry it out in the way that he would carry out a research of the finest type. I see all the students who take the Higher School examinations in the University of London, about 4000 students or so, and I find regularly that when taking this experiment a student connects the apparatus up, finds the balance point, and works out the result, and that is that. That is not sufficient. My point is that he should be able to indicate the kind of accuracy that he has obtained, and if that is not taught, then I think that the teaching is at fault.

Prof. W. Wilson. I should like to express my disagreement with the views of two or three gentlemen who have maintained, if I have understood them correctly, that you should teach physics of rather a different kind to the boy who is not going to be a scientist or a physicist but is going out into commerce or practical life. I entirely disagree with them. I think that what is desirable to be taught to one who intends to be a physicist is of immense benefit to every boy who takes an interest in science.

Mr R. S. Maxwell. Various speakers seem to imply that if we adopted pantopiric examinations the laboratory course would be altered. I am afraid they must have rather a low opinion of human nature if they seriously maintain that, because surely the laboratory course is independent of any examination, and most universities insist that a certain laboratory course should be shown to have been performed when the candidates send in their note-books.

Mr Meier and Mr Dark added a very excellent suggestion that there should be two kinds of practical examinations. Two papers should be set testing different things, one pantopiric, one ordinary, but in that case I should like if possible to stipulate that in the ordinary type of examination there should be no choice of questions; everybody should do the same question. I think that is rather important.

With regard to Mr Platt's point about nervousness, we do not find the candidates nervous when once they have settled down. They adapt themselves very quickly to the conditions.

We have not made any attempt to form our own opinions of the candidates first of all and then to correlate with the pantopiric marks.

Prof. J. C. Philip (Chairman). Personally I should like to do without practical examinations at all. I should be inclined to go on the pupil's record, not to depend on the result of any practical examination. That may be, of course, an unattainable ideal, and it has been very interesting this afternoon to hear different points of view expressed. The suggestion made by Dr Richardson and Mr Maxwell is perhaps of a rather revolutionary character, and I have no doubt that as Mr Meier has already put the scheme into tentative operation, many of you will take the opportunity of trying out something on the same lines. It is all to the good, if we can do it, that these variants on the stock methods of examination should be tried.

I am sure we are glad to thank the men who have opened the discussion and have read papers to us, and all who have contributed to it.

We are grateful also to the Physical Society for giving us this opportunity of airing a very important matter.

COMMUNICATED REMARKS

Mr J. R. Clarke (University of Sheffield). Prof. Wilson states the greatest difficulty with respect to practical physics examinations when he says that the examinations have become an end in themselves. On account of apparatus limitations the number of experiments which can be set on a given syllabus (especially in electricity and magnetism) to a large number of candidates is very restricted. There is, therefore, a tendency on the part of examinees and their instructors to concentrate on these standard experiments, such as the comparison of resistances by the meter bridge. It is true that this concentration may lead to the fulfilment of one of the purposes of practical physics training, namely, the development of the ability to make accurate measurements. On the other hand, it frequently happens that if a candidate so trained be asked to make a deduction from his observations, other than that to which he has been accustomed, he is unable to do so.

It seems to me that if the pantopiric examination devised by Prof. Richardson and Mr Maxwell were adopted there would be even less incentive to train students to be able to use their observations than there is at present. This type of examination is open to other objections. One school which I have examined yearly in practical physics for the Northern Universities School Certificate presents about 80 candidates, and this necessitates four or five sessions each year. The difficulty of conducting a pantopiric examination, even for one year, and using several of the questions at more than one session will be appreciated, but I think it would be impossible to conduct it for several years without presenting the physics master with a large number of "certainties."

The system adopted by the Northern Universities Joint Matriculation Board in their optional practical physics test is the most satisfactory I know, except the internal examinations of the universities where considerable weight can be given to the opinion of the demonstrators who have observed the candidate for two or three years. The Board instruct their examiners to regard sixteen as the normal number to be examined in a $2\frac{1}{2}$- or 3-hour session. Every candidate is required to carry out two experiments, without choice, but the limitation of the number of boys enables the examiner to assist a candidate who is "at sea" and, if desirable, to change his experiment. There is also time to ask a boy who is doing an electrical experiment one or two questions on practical magnetism, for example. Nearly all the experiments set involve three or more measurements and usually these have to be used in a way designed to test the boy's intelligence, though the measurements themselves are quite clearly indicated. If the examiner obtains the opinion of the master as to which boys are on the border-lines between passing and failing or passing with distinction, he can pay a little extra attention to them, and even a hasty perusal of laboratory note-books assists in the final placings.

Mr J. A. TOMKINS (Leeds). I have not had time to study the papers very fully, but I have been much interested in the paper describing a new type of practical examination. It does seem to possess the two advantages claimed, viz. that it is a fair competition and that the tests can be spread fairly over the whole syllabus. For some years past the practical examinations in physics held in the Bradford Technical College have, wherever possible, been of the kind fulfilling the first condition, that is, all the students have been given the same experiment, but these have been of the usual type and not the short experiments described by the authors. Thus in a class of intermediate standard two experiments are set to be done in 3 hours. Where the number of candidates exceeds the number of pieces of apparatus, half the students work on each experiment and they change over at "half-time." One objection to the new method appears to be that pointed out by Prof. W. Wilson. It is very doubtful whether ten or more short experiments call for the same sustained effort and attention and the train of thought involved in one or two longer experiments of the usual type. This point is referred to by the authors in their paper and, perhaps, can only be settled by further investigation. A further slight disadvantage seems to be implied in the authors' remark that "the candidates usually looked rather tired at the end," but this probably holds to some extent for all types of examination.

DEMONSTRATIONS AND EXHIBITIONS

A Model Four-wheeled Vehicle caused to Skid by the Application of the Brakes. *Demonstration given on November* 22, 1929, *by* J. BRADLEY, B.A., The National Physical Laboratory.

The model used for the demonstration had rubber-tyred wheels 4 in. in diameter and fitted with independent brakes. It could be projected along the floor by a simple form of catapult and any desired combination of brakes could be applied. It was shown that the model tended to turn round when the back wheels were locked but continued in a straight line if the front wheels or all four wheels were locked. This effect was also illustrated with other small models projected by hand along the table. One of these models was arranged to show how in the case of a back-wheel skid the driver could avoid turning round by holding his front wheels parallel to the direction of travel of the centre of gravity of the car.

A New Periodic Effect in a Neon Discharge Tube. *Demonstration given on December* 13, 1929, *by* Dr W. A. LEYSHON.

Observations were made recently with a cathode-ray oscillograph on the shape of the discharge-current/time curves in a number of beehive Osglim lamps arranged in turn in a "flashing" circuit. It was noticed that in the case of two lamps having cathodes consisting of more open spirals than the others, the times taken to reach the maximum current in the discharge were greater. It was suggested by Dr W. H. Eccles, F.R.S., that the discharge might "creep" up the cathode, and that it might be possible to observe the movement by a specially devised stroboscopic method. It should be remarked that the order of time concerned in the rise of the discharge to a maximum value was 10^{-5} seconds.

It was thought that the effect might be accentuated, and therefore might be easier to detect, if the cathode were very much larger than the anode, and if the latter were placed as asymmetrically as possible with respect to the former. The two I-type lamps shown were made up in the research laboratories of the General Electric Company; in each the anode is about 4 mm. long and is placed opposite the lower end of the cathode, which is of the dimensions usual in this type of lamp. The lamps were filled with pure neon at a pressure of 14 mm. of mercury.

During an attempt to obtain the static volt/ampere characteristics of these lamps in the usual way, it was found that for certain values of voltage and current small periodic fluctuations occurred, the cathode glow and the anode glow undergoing changes in position simultaneously with the fluctuations. When the two glows were nearest to each other the current was greater and the voltage less than when they were more separated. The actual changes in current were in general a small fraction of the total current passing through the tube, and they took place quite suddenly.

The movement of the glows could be followed by eye, the periodic time being of the order of ½ min. in most cases. In one type of oscillation it was noticed that the top of the cathode glow crept up the electrode and, when it just reached the top, the whole glow contracted suddenly to the top of the cathode, the anode glow moving to the far side. The change in current occurred at the instant of contraction. The glow now stayed at the top of the cathode for some time, very gradually creeping downwards, and then suddenly dropped, the current suddenly rising. Sometimes the movement was in two stages, the glow waiting for some seconds half way up the cathode before going on its upward or downward way. The current here had an intermediate value. Other types of movement were noticed with different values of current through the lamp.

The phenomenon was observed originally with a high variable resistance, of the order of 100,000 ohms, in series with the neon tube. When this resistance was slightly reduced the periodic time of the fluctuations was diminished. A similar effect was obtained by increase of the applied potential. Later, a saturated diode (triode with grid and anode connected) was connected in series to control the current. This arrangement was found very convenient. In one experiment, heating the tube slowed up and finally stopped the oscillations; cooling it by resting it on a block of ice increased the frequency. One tube gave rapid flickers with comparatively large fluctuations in current when near the extinction point. It was found that a transverse magnetic field in one direction (such that an electron current would be urged upwards) increased the frequency; in the opposite direction it reduced the frequency. The anode glow moved rapidly from one part of the anode to the other, changing in appearance as it went.

For certain circuit conditions a note was heard in telephones included in the circuit, generally when no periodic movements of the glow were being observed. In one case the note gradually fell in frequency, and when it could no longer be heard the glow suddenly contracted. The glow then gradually spread out again and jumped upwards, and the process was repeated. The phenomenon may well be associated with the production of ionic oscillations. The observed movements of the glows and the changes in current indicate that the periodic occurrences observed are some form of relaxation oscillation.

It is hoped to examine the phenomenon in more detail in the future.

A Method of Showing the Phase Relation between the Flash and the Fork in a Fork-controlled Flashing Neon-tube Circuit. *Demonstration given on December* 13, 1929, *by* Dr W. A. LEYSHON *and* Miss TERESA M. DILLON.

The fork-controlled flashing neon-tube circuit was first described by Dr W. H. Eccles and Dr W. A. Leyshon*. Later, a theoretical and experimental investigation of the controlling action was made, a cathode-ray oscillograph being used†. It was shown that the fork would attain its maximum amplitude when its effect on the period of flashing was greatest, the period being shortened by the reaction of the fork. Further, in this condition the flash occurred when the fork was in a position of maximum displacement away from the attracting coils. It was also shown that if the circuit conditions were altered within a suitable range so that the frequency of flashing without control would be rather greater, the fork could still be maintained with diminution of amplitude and alteration of phase.

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* "Mechanical and Electrical Vibrations," *The Electrician*, **97**, 65 (1926).

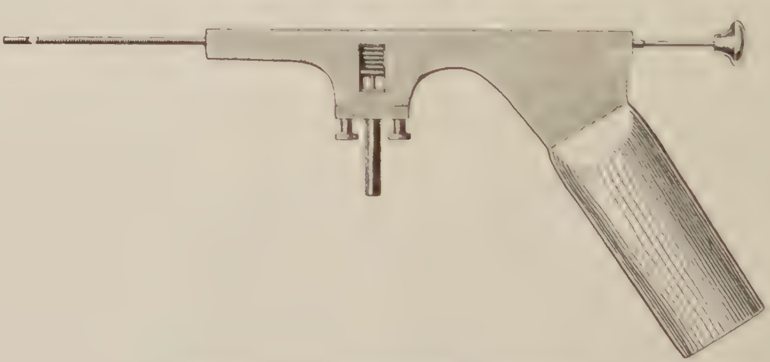
† "On the Control of the Frequency of Flashing of a Neon Tube by a Maintained Mechanical Vibrator," *Phil. Mag.* **4**, 305 (1927).

In the present apparatus light from the neon lamp is received on the objective of a microscope. Part of the field is obscured by one prong of the tuning fork, which in this apparatus has a frequency of 100 ~. The microscope is focussed on the upper surface of the prong, and when the fork is being steadily maintained by the flashing circuit the dark edge appears to be quite steady. Its position alters laterally with any change in the phase relationship. Light from a small lamp is focussed on the edge of the fork, so that a number of bright spots of light can be seen when the fork is stationary. These are seen as bright lines when the fork is vibrating, their length giving the amplitude of the vibration. The position of the dark edge with respect to these bright lines is an indication of the phase. When the fork is vibrating with maximum amplitude it is seen that the dark edge is at the end of the bright lines corresponding to the position of the fork furthest from the coils. By this simple means the corresponding changes of amplitude and phase can be observed and measured.

The apparatus also shows visually the non-uniformity of beats which occurs when the fork has not yet attained sufficient amplitude to pull the circuit into tune.

Apparatus for use in a Hospital Radium Service *exhibited on December* 13, 1929 *by* Prof. F. Ll. Hopwood, D.Sc.

Radium is used in hospital practice in two forms. Solid radium salt ($RaSO_4$) is enclosed in hollow needles made of platinum; while radon or radium emanation



Instrument for implantation of radon seeds.

is pumped off from a solution of radium bromide in acidified distilled water, purified, and sealed into minute glass capillaries which are in their turn inserted into platinum jackets. Radium needles vary in length from 1 to 6 cm., and radon "seeds" are usually about 6 mm. long.

Tests for leakage of needles are carried out by means of an α-ray electroscope, and the strength of needles and "seeds" is measured by means of a γ-ray electroscope due to Hollweck.

In addition to the above-mentioned apparatus, special protected carriers, and a microscope designed for the rapid visual examination of radium containers were exhibited.

Finally various forceps for handling and inserting needles were exhibited and also a "gun" (see illustration) which is used for the insertion of radon seeds into patients.

BOOK REVIEWS

The Effects of Moisture on Chemical and Physical Changes, by J. W. SMITH, B.Sc., Ph.D. (London: Longmans, Green & Co. Text-books of Physical Chemistry edited by Sir William Ramsay and F. Donnan.) 15*s*.

For many years past the action of minute traces of water in catalysing chemical changes has been a matter of considerable interest. There was, however, no connected account of the subject available, and, in order to comprehend it, it was necessary to read through most of the original literature. The above book will be extremely useful to those who wish to know something of the work that has been done on intensive drying, but have no time to adopt the above procedure. It can also be recommended to the research student, as it is an excellent guide to the original literature.

The latter part of the book will, probably, be the more interesting to the physicist. It deals with the changes that have been observed in boiling point, vapour pressure, density and other physical properties when the last traces of water are removed from pure substances. The theories of molecular association that have been put forward to explain these changes are discussed. It is a pity that the author was unable to indicate the relative merit of the work described in this section, since there is a certain amount of contradiction in the results obtained by different workers.

In a chapter on dissociating compounds, it is stated that the accepted value for the vapour density of ammonium chloride is that obtained by Smith and Lombard which corresponds with 60 per cent. dissociation. This suggests that the results of H. B. Baker were incorrect, for he found that normally the vapour was completely dissociated but that when intensively dried it was practically undissociated. Actually, Baker's experiments are confirmed in two recent papers—Braune and Knoke, *Z. physikal. Chem.* **135**, 49 (1928); Smits and de Lange, *J. Chem. Soc.* **48**, 2499 (1928)—where it is conclusively shown that the undried vapour is completely dissociated.

Gaseous Combustion at High Pressures, by WILLIAM A. BONE, D.Sc., LL.D., F.R.S., DUDLEY M. NEWITT, Ph.D., D.I.C. and DONALD T. A. TOWNEND. Pp. xiii + 396, with 148 illustrations and 14 plates. (London: Longmans, Green & Co., 1929.) 42*s*. net.

This is a companion volume to the book on *Flame and Combustion in Gases* published in 1927. It tells the story of the combustion of hydrogen-air, carbonic-acid-air and methane-air mixtures at various initial pressures between 3 and 175 atmospheres. All this has made necessary a very special technique which has been very largely developed in Prof. Bone's laboratories at South Kensington. The first seven chapters are devoted to a description of these laboratories and their special equipment for high pressure work. Eight chapters devoted to the characteristics of explosions, including a discussion of the much discussed "activation of nitrogen in carbonic-acid-air explosions" follow, and then come chapters devoted to the distribution of energy, the condition of the medium at the moment of maximum pressure, the calculation of mean maximum pressure, the heat capacities of the gaseous products and the influences of pressure and temperature upon the explosion ranges of gaseous mixtures. Most of this work is very recent and a good deal is extremely controversial. Practically all the experimental results have appeared in either the *Proceedings* or the *Transactions of the Royal Society* since 1915. The data contained in the original papers are now superseded by "corrected" data contained in the present volume. So far as the investigator is concerned, that would appear to be the main justification for the appearance of the volume at this time. A good deal of its matter is of the nature of *unreife Frucht*. What, for instance, shall we say of all the pother about the activation of nitrogen in carbonic-acid-air explosions? Industrial concerns here and abroad are avid for processes of nitrogen fixation. Would they venture a brass farthing on processes based upon the experimental results and theory contained in the volume? We think not; they would at least look on both sides of their coin before parting. Industrialists ask for nitrates and are given *p/t* diagrams. Evidence of the combination of nitrogen and oxygen, to a small extent, was obtained in Cavendish's original experiment of sparking a mixture of oxygen and nitrogen. Is there any evidence that the amount of "activation" or combination occurring in Prof. Bone's experiments is any greater, and such as could be detected chemically by anything but the most delicate reaction? If there is, it has escaped our notice. The book is one of considerable value for the general scientific worker and technologist, more especially those concerned with the preparation and handling of gases. These will find the chapter on the preparation, storage and compression of gases very valuable. Why our old, familiar, tried and trusted Bone and Wheeler gas analysis apparatus, modified principally by the use of hydraulic pressure for controlling the mercury level—a device due to Stockings—should be called the Bone-Newitt apparatus is an inscrutable riddle. What's in a name? We note that the Wheeler-Payman "supposed" law of flame speeds is given its *congé* on p. 117. The book is well got up, and is very well printed on good paper; adequate name and subject indexes are included. Future editions would be improved by reference to the work of Haber and Wolokitin on the oxidation of nitrogen in the hydrogen flame and to the work of Garner, Lewis, Canby and Woodbury. And still the occurrence of what may be called "South Kensington myopia" in authors who have felt "reluctance in writing this volume because of its necessarily being devoted chiefly to our own researches" is excusable, if perhaps regrettable. The book is not unreasonable in price.

J. S. G. T.

The Kinematical Design of Couplings in Instrument Mechanisms, by A. F. C. Pollard, A.R.C.S., A.M.I.E.E. Pp. 64. (Adam Hilger, Ltd.) 4*s.* 6*d.* net.

It is pleasing to find a writer on the subject of mechanisms who prefaces his essay with a reference to principles laid down by Willis, Kelvin and Clerk Maxwell and so approaches the discussion of the correct design of couplings from the only logical standpoint, and this to good purpose. This mode of approach, as the author of the present little monograph explains clearly in his earlier pages, consists in obtaining first a clear conception of the number of degrees of freedom required in the coupling or connection and then in an examination of the means by which the necessary number of constraints and no more may most conveniently be provided.

After a brief but adequate treatment of the subjects of variance and accuracy in mechanisms and a necessary classification of couplings according to their degrees of relative freedom, there follows the principal section of the book, devoted to a number of well-chosen examples of the application of correct kinematical design in actual instruments by well-known makers, all of which are admirably explained. The book closes with a short section on procedure in design, expressed in rather general terms, and it is to be hoped that the author will in a future edition develop this important aspect of the matter more fully.

The need for such a book as this is obvious enough, for there are few experimenters who have not had to say hard things about the performance and the adjustments of many of the instruments which they handle. This well-printed and lucid little treatise must therefore be commended to all users and designers, especially of surveyors' instruments, as well as to the much wider circle engaged upon other kinds of mechanical designing, seeing that it draws attention to fundamental principles of kinematics which are too often neglected or ignored.

E. H. L.